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Retirement Responses to a Generous Pension Reform: Evidence from a Natural Experiment in Eastern Europe‡

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Abstract

The retirement decision is under researched in developing and emerging countries, despite the topic's close relation to many development issues such as poverty reduction and social security, and despite the fact that population ageing will increasingly challenge the developing world. This paper uses a natural experiment from Ukraine to estimate the causal effect of a threefold increase in the legal minimum pension on labor supply and retirement behaviour at older ages. Applying difference-in-difference and regression discontinuity methods on two independent nationally representative data sets, the paper estimates a pure income effect that caused additional retirement of 30 to 47 percent. Additional evidence suggests that retirement incentives are stronger at the lower tail of the educational distribution and that the strict Labor Code curbed responses at the intensive labor supply margin. Although the substantial pension increase provided strong disincentives to work and put a heavy fiscal burden on Ukraine, it significantly reduced the propensity of falling into poverty for those in retirement.

Keywords: Labor supply, retirement, minimum pension, pure income effect, poverty, difference-in-differences, regression discontinuity

JEL: J26, I38, O15

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1. Introduction

The retirement decision is under researched in developing and emerging countries, despite the topic's relevance for many development issues such as poverty reduction and social security, and despite the fact that population ageing will increasingly challenge the developing world. This paper analyses the question how increasing generosity of old-age pension provision impacts on the retirement decision at older ages in a poor country. A unique natural experiment allows disentangling the pure income effect from pension generosity in unusual clarity. Estimates of the income effect of labor supply are valuable as many poor countries see the improvement of pension systems as a crucial tool in the fight against poverty (cp. Holzmann and Hinz, 2005; Barr and Diamond, 2008).

In developing and emerging countries, insufficient old-age income provision is paired with underdeveloped or missing financial markets hampering private pension provision.[1] Although a number of emerging countries have successfully introduced non-contributory pensions with broad coverage (Willmore, 2007; Barr and Diamond, 2008) very little is known about the labor market and retirement effects of pension systems in the developing world.[2] In contrast, the potential disincentive effects for the labor supply of older people is well-documented for many industrialized countries (Gruber and Wise, 2004), although the literature remains ambiguous about the impact of social security systems on labor supply behaviour (e.g. Moffitt, 1987; Krueger and Pischke, 1992). On institutional grounds, Freeman (2009) reviews some recent evidence on the pass-through of pension contribution rules on labor costs and labor demand in a number of developing countries. Barr and Diamond (2008) discuss some pension and retirement features for developing countries like relatively low retirement ages, widespread use of early retirement and the coverage problem of the informal sector. However, behavioural responses to cash transfers and retirement rules in developing countries are even less researched. Probably the best studied country is South Africa, where the availability of good cross-sectional and (lately) panel data has stimulated research on various aspects of labor supply and income pooling of the old-age social pension (Bertrand et al., 2003; Duflo, 2003; Ardington et al., 2009); however, all the papers deal with the labor supply responses of working-age adults in multi-generation households. McKee (2008) focuses on old-age labor supply in response to family transfers in Indonesia and simulates the

[1] Where they exist in the developing world, pension systems are mostly characterized by insufficient replacement rates and low coverage due to poor administrative capacities, informality and wide-spread self-employment.

[2] The small retirement literature contrasts with an increasing literature on labour market regulations and their effect on labor market outcomes in developing and emerging countries (e.g. Harrison and Leamer, 1997).

potential welfare gains from a defined-contribution system. The only direct evidence on retirement responses to social security receipt is the evaluation of a multi-faceted change in the pension eligibility rule for rural workers in Brazil (de Carvalho Filho, 2008). A simultaneous change in several pension eligibility criteria—among them a doubling in minimum benefits—reduced male labor supply by roughly 38 percentage points.[3] Costa (1995) provides evidence on a pure income effect from the turn-of-the-century Union Army Veteran Pension; however, pension receipt was then based on health status.

This paper exploits the exogenous income variation provided by an unforeseen departure from the pension reform track, upon which the government of Ukraine had embarked the country. Ukraine is a lower middle income country with a GDP of 5,300 USD per capita PPP in 2003 (comparable to Peru and China) equalling 14 percent of the US level. In 2002 a comprehensive pension reform had been approved in order to reduce the fiscal burden of the pension system, which has always been characterized by full coverage of the population and low retirement ages. In September 2004, reform objectives were suddenly changed with probably one of the world's largest pension increases being implemented. Pensioners in Ukraine saw a threefold increase in the legal minimum pension, resulting in an almost universal flat benefit rate which was paid out upon reaching retirement age without any means or retirement testing. In early 2005, almost all pensioners in Ukraine received exactly the new minimum pension benefit which amounted to roughly 65 USD.

The estimated labor supply and retirement effects have a causal interpretation by comparing the retirement behaviour of those slightly above the statutory retirement age before and after the pension increase (the treatment). The counterfactual is given by those slightly below retirement age. As old-age pensions are neither means-tested nor conditioned on retirement, the rise in benefit levels will induce a pure income effect, which enables an individual to afford more leisure (under the assumption that leisure is a normal good) (cp. Costa 1995). After controlling for trends in general labor supply over time, the coefficient on the interaction between the treatment group dummy and the treatment indicator reflects the income effect of the pension increase.

The paper offers three contributions: First, it carefully identifies the labor supply response to a substantial pension increase at the extensive and intensive margin using a Difference-in-Difference as well as a Difference-in-Regression-Discontinuity design. The

[3] One disadvantage of the Brazilian data is that the type of pension benefits (old-age, disability, social assistance) cannot be accurately determined. Different from the Brazilian reforms (de Carvalho Filho, 2008), the current analysis can also rule out incentive effects from additional years of services. As benefits do not depend on contributions the individual retirement decision won't be confounded by the change in prospective pension accruals.

robustness of the results across two independent data sources, different estimation methods and a number of sensitivity tests is a reassuring indicator not only in a developing country setting. Second, this paper presents results for an emerging country where the entire population is affected from a change in the pension legislation. Ukraine is a lower-middle income country[4] and thus represents the group of countries containing the majority of the global population (including countries like China, India, Indonesia, Pakistan, and Nigeria). The paper thus adds evidence beyond the pre-existing small literature on higher-middle income countries and complements analyses on pension changes that apply to some population subgroups only. Also, the paper offers some evidence on the magnitude of the poverty reduction induced by the reform. Third, unlike much of the earlier literature, this paper analyses retirement decisions of both, men and women.

The results of this study indicate that higher pension incomes have strong disincentive effects on the labor supply decision of elderly people. The income effect from the new pension policy leads to a 37-47 percent increase in retirement at the statutory retirement age for men—and to a 30-39 percent increase for women. Those women who remain in the workforce reduce their yearly working hours by 15 percent, while men have no significant response at the intensive labor margin. The estimated effects are substantially stronger for less educated than for better-educated. As retirement effects are estimated purely on age eligibility, these figures can be regarded as lower bound estimates. While the respective retirement elasticities with respect to pension income are somewhat lower than previously reported in the literature (at 0.32), elasticities estimated on actual benefit receipt are twice as large. From a welfare perspective, the pension increase has significantly reduced elderly poverty in absolute terms, but has also improved the old generation’s position relative to the working age population.

The remainder of this paper is organized as follows: Section 2 describes the data set, the main features of the Ukrainian pension system including details on the generous pension increase of the year 2004. Section 3 discusses the identification strategy used in this paper and presents the main retirement and labor supply results with several robustness tests. Results concerning absolute and relative deprivation are provided in Section 4. Section 5 concludes with some implications for public policy.

[4] According to the World Bank‘s Atlas method.

2. The Legal Minimum Pension Increase in Ukraine

Pension Reform

Ukraine has a mandatory defined benefit state pension system which is in practice exclusively based on qualification by age. It covers all Ukrainians who have worked for at least 20 (women) or 25 years (men) and who have reached retirement age. By international comparisons, the state pension age is low with women qualifying from age of 55 and men from age of 60.[5] For the near future, the system will resemble a non-contributory pension scheme, as those Ukrainians close to retirement age have accumulated most of their employment histories during the Soviet era and in a labor market that was characterized by full employment and high wage compression. Consequently, coverage of the system has been almost universal.

In the early 2000s, the Ukrainian pension system was characterized by an extremely high level of benefit compression. Pension benefits had been capped at three times the legal minimum wage (plus minor additions) resulting in an almost flat pension rate (Noel et al., 2006). At the same time the state pension scheme offered at minimum pension guarantee to support those who receive low benefits.

In 2002, the government discussed and ratified a comprehensive pension reform which aimed at better incentives for later retirement (by paying additions for pension deferral) and for compliance in contribution payments of high-income earners (by removing the pension cap) in order to ease the fiscal strain of the system.[6] The reform came into force in January 2003.

In September 2004, however, the Cabinet of Ministers suddenly increased the minimum pension level per decree in an attempt to reduce poverty among the elderly.[7] In nominal terms, the guaranteed floor rose from around 100 Ukrainian Hryvnia (UAH) per month to over 280 UAH in late 2004 and almost 350 UAH (roughly 65 USD) in early 2005 (Figure 1). As minimum wages did not keep track of this rise, the guaranteed minimum pension even exceeded the legal minimum wage after September 2004. While the general

[5] There are several hazardous occupations in which the normal retirement age is below the stated values, e.g., in mining. Similarly low retirement ages prevail in China, most transition countries, but for instance also in Colombia and El Salvador.

[6] The future pension system was designed to rest on three pillars, with the first one resembling a mandatory pay-as-you-go state pension system, the second one being a mandatory individual pension and the third one being private pension insurance. The second pillar was scheduled to start after 2007, while the other two pillars were scheduled for 2003 (for details see Handrich and Betliy, 2006). Contributions for the PAYG system are made by employees (1-2 percent) and employers (32 percent). Fiscal imbalances are smoothed out by budget subsidies.

[7] CM Decree on Improving the Pension Provision Level, No.1215.

reform had been designed to remove the cap on the state pension, the sharp rise in the minimum pension introduced a binding pension floor: Average wage earners with 40 years of working history suddenly received no more than the minimum pension, and consequently 88 percent (!) of the 13.3 million pensioners in Ukraine received a flat benefit rate (World Bank, 2005). Albeit at a higher absolute level, overall benefit compression had further increased (Figure 2).

Even for the national pension fund, which had to administer the change, the government's step to increase the minimum guarantee level came as a surprise. In previous months, the fund had struggled with the government about insufficient transfers from the State Budget and threatened to no longer pay out benefits. As the institutional ambiguity in the financing of pensions became increasingly public, many people might not even have expected to receive their full pension benefits in mid 2004. The sudden pension increase was implemented without following the usual legal procedures in time and the government codified the higher pension rights only in April 2005 by amending Article 28 on the "Minimum old age pension" of the State Pension Law.[8] The abruptness of the pension rise is well documented and most observers immediately expressed concern that this step might thwart the government's reform attempts (Kotusenko, 2004; World Bank, 2005; Gora, 2008).

The timing of the pension increase just few months before the general elections generated rumours that the government had recognized pensioners as a powerful electorate (Handrich and Betliy, 2006). Pensioners have often been considered the losing generation of the post-Socialist transition process. However, contrary to the public perception, there is no empirical evidence pointing to pensioners being more poverty exposed than other social groups in Ukraine or Russia, especially when measured in terms of consumption (Mroz and Popkin, 1995; Brück, Danzer, Muravyev and Weisshaar, forthcoming).

Official data give a first impression of the effect of the pension increase at the aggregate level. According to the State Statistics Committee of Ukraine, the share of pensions in total household resources stagnated around 23 percent between 2000 and 2003, and then jumped by five percentage points in and after the reform year 2004 to remain relatively stable thereafter. As will be discussed later, aggregate data might mask household composition effects so that the substantial increase of pensions in total household incomes does not

[8] The amendment reads as follows: „From 12 January 2005, in accordance with an earlier implemented change to Article 28 of the Ukrainian Law „On Mandatroy State Pensions Insurance", the provision of the minimal old-age pension, which applies from a minimum of 25 service years for men and 20 service years for women, will be adjusted to the subsistence minimum which applies for persons who have lost their income generating capacity (332 UAH)." (Ministry of Labor and Social Policy, 2006, 36)

necessarily reflect the pure effect of the pension increase.[9] Given this generous pension increase allied to a progressively ageing population, the fiscal burden of old-age pensions on the public budget in Ukraine became substantial.[10] Total expenditures on the pension system increased from nine to 15 percent of GDP between 2003 and 2005 (Gora, 2008: 34). The comparable figure for the OECD average was 7.2 percent of GDP in 2005 (OECD, 2009: 138) and even countries with very mature pension systems like Germany or France have shares of around ten percent. Although the fiscal burden and demographic challenge of the Ukrainian pension system might seem obvious, its costs have to be understood in the light of its achievements (see Barr and Diamond, 2008). Consequently, this study also aims at analyzing whether the pension increase has achieved the announced public policy objective of poverty reduction.

However, the main contribution of this paper lies in the analysis of unintended labor supply consequences of the reform. In comparison to industrialized countries, the shares of working pensioners are high in Ukraine. Two years above statutory retirement age (i.e., at 62 and 57 years of age), roughly 40 percent of men and women have regular employment, and that share halves within the next three years. Traditionally, the phenomenon of working pensioners was attributed to the insufficient pension entitlements of many elderly, as evidenced for Russia (Kolev and Pascal, 2002). Additionally, working relations are still inflexible in Ukraine and most individuals face the choice between working full time or not at all. As a consequence, labor supply responses in Ukraine take predominantly place at the extensive margin and people might work more than actually desired. If poverty was the cause of the elderly staying at work, a significant non-anticipated pension increase like the one in 2004 should allow more pension-aged to afford retirement without falling into poverty.

[9] Those composition effects can only stem from changing co-residency patterns of households and not from population ageing per se, as the share of pensioners in the total population remained roughly stable over the period under consideration.

[10] There have been debates about increasing the statutory retirement age, however, the Ukrainian Prime Minister Yulia Tymoshenko announced on her private homepage in 2007, that no such increase would be introduced due to the low life expectancy of the Ukrainian population.

3. Retirement response to the pension increase

Data

The analysis is based on several cross sections (2002-2006) of the national representative Ukrainian Household Budget Survey (UHBS) which interviews 25,000 individuals on an annual basis. Data collection is performed by the State Statistics Committee of Ukraine in December of each year. The data comprise a rich set of individual and household characteristics, information on employment as well as incomes. A drawback of the data set is the way how earnings and pensions are retrieved. Individuals are asked to report net yearly earnings and pension benefits. As a consequence, the effect of a pension increase in late 2004 will show up only partially in the December 2004 data. Consequently, December 2005 values are used as post-reform observations, since the pension increase was only fully reflected in the 2005 wave. Unfortunately, the UHBS lacks information on working hours; however, the persistent structural inflexibility of the Ukrainian labor market allows little choice at the intensive margin of labour supply. Consequently, most workers are contracted full-time with 40 hours per week, and the reduction of working hours is constrained by the vast majority of employers who are reluctant to provide part-time jobs. More than sixty (almost fifty) percent of employees worked exactly 40 hours in an average (the reference) working week and the concentration on full time is even more pronounced for those working beyond retirement age (Figure 3). The pattern is similar for men and women and there is no significant change between 2003 and 2007.[11]

The analysis of working hours is feasible in the Ukrainian Longitudinal Monitoring Survey (ULMS), a panel data set which is complementarily used to overcome some of the data limitations of the UHBS. The nationally representative ULMS is collected by the Kiev International Institute of Sociology. All three waves of the panel (for the years 2003, 2004 and 2007) are used in the analysis. As the vast majority of data collection is performed in early summer (May to July), the panel comprises two waves prior to and one wave after the exogenous pension increase. The data set allows a comprehensive analysis of labor market responses as it contains information on working hours, number of working weeks as well as monthly net incomes. Also, the use of panel data allows us to control for unobservable

[11] The share of those working between 15 and 25 hours is higher among working age women (7 percent) than among working age men (3 percent) and higher among pension aged women (12 percent) than among pension aged men (8 percent).

individual characteristics which might impact on labor supply behaviour in a way that is non-traceable when using cross-sectional data.

The main variable of interest in the analysis will be the retirement status based on an activity-income-centred definition. A person is retired if not working, receiving old-age pension benefits and subjectively self-categorizing him- or herself as retiree. Labor supply intensity is measured in hours per year, weeks per year and hours per week. A detailed description of variable definitions in both data sets can be found in Table 1.

Identification Strategy

In Ukraine, one can draw a pension upon reaching retirement age (55 for women and 60 for men). Legally, the second requirement for pension eligibility is a minimum of 20 (women) or 25 (men) years of work. The UHBS data set contains information on total working years, i.e. the years worked throughout lifetime, which shows that only a minor fraction of those reaching retirement age has worked fewer than the required 20/25 years as a consequence of the Soviet full-employment policy (1.9 percent of women and 2.0 percent of men).[12] In order to maintain a purely exogenous pension age indicator, all presented results are not conditioned on the minimum working years requirement.[13] Consistent across both data sets and all years, the share of pension aged exceeds the share of those receiving an old age pension by one to two percentage points. Beside pensions arrears (which were almost negligible during the observation period), the difference mainly stems from pension aged individuals who kept working without drawing the compulsory state pension, for instance, if they were not registered at their current place of residence. To circumvent potential selection bias into actual pension receipt of the elderly the following analysis uses age-based pension eligibility as an instrument for actual benefit receipt.

The identification strategy of this paper exploits a natural experiment in Ukraine. Using the unanticipated minimum pension increase as a treatment to those receiving a pension, the income effect on labor supply choices and retirement behaviour of those close to pension age can be interpreted as a causal effect. Figure 4 and 5 show retirement rates for one year prior and one year post pension reform on the y-axis. The full dots mark the year 2003 while the circles stand for the year 2005. On the x-axis, age is reported with a vertical line

[12] Actually one would prefer to have a measure of years with pension contributions. Although informal sector employment might be substantial in current Ukraine, the largest fraction of those close to the retirement age have reached the minimum year requirement already during Soviet times. For instance, men born in 1944 who had started working in 1964 had already 27 years of working experience when the Soviet Union broke apart in 1991.

[13] Robustness checks excluding those with below 20/25 years of work experience from the eligibility criterion indeed confirm that the true effect is economically and statistically slightly bigger (see robustness check 2 of Table 25).

marking the gender-specific retirement age. The fitted values in the graphs are predicitions from weighted polynomial regressions. For both, men and women, we observe some early retirement to the left of the retirement discontinuity. The differences in the predicted values are modest below pension age. Above retirement age, however, there is an apparent upward shift in retirement rates after the reform year 2004. The discontinuity exactly at the retirement age has widened significantly between 2003 and 2005. This gap (and not the change from below retirement age to above retirement age) is the retirement response of the minimum pension increase of 2004.

Difference-in-Difference estimation

The Difference-in-Differences (DiD) estimator exploits the discontinuity in pension eligibility at retirement age to compare changes in outcomes between those eligible (treatment group) and those not yet eligible (control group) for an old-age pension over time. Keeping in mind that the analysis is purely based on pension eligibility, the presented effects have to be understood as lower bound estimates of the true effect. The treatment of interest is the threefold increase in benefits and we are interested in its impact on the outcomes of interest, retirement and labor supply intensity in the treatment group. As a pure before-after comparison of outcomes among the treatment group might be affected by time specific factors that are common to all workers, the control group is used to difference away general trends in retirement behaviour, e.g., changing macroeconomics conditions and aggregate labor demand. The timing of the pension increase allows using two cross sections of the UHBS before and after the reform, however, to prevent from other potentially confounding factors, the analysis is cleanest when performed on two cross-sections before (2002/2003) and one cross-section after the pension increase (2005). Table 2 shows the identification strategy by mean comparisons in two-by-two matrices. The upper panel indicates that women exhibit lower retirement rates than men across all cells. Also, the retirement effect of reaching retirement age is stronger for men (47 percentage points) as compared to women (44 percentage points). The time trend for those below retirement age is (insignificantly) negative, reflecting the increasing labor force participation during the growth period of the mid 2000s in Ukraine. However, for those above retirement age, the time trend runs in the opposite direction, leading to an even larger treatment effect of 17.6 percentage points for men and 13.3 percentage points for women. Caused by the pension increase, retirement rates rose by 37 and 30 percent. The two lower panels report results from two falsification exercises, the first one simulating an artificial retirement age at 58 (for men) and 53 (for women) and the second simulating the pension increase between the years 2002 and 2003. The first control experiment indicates that early retirement rates generally increased with age but remained fairly stable over years. The negative time trend at younger ages reflects the general positive employment trend. Control experiment two shows that changes between 2002 and 2003 were modest and insignificantly different from zero. The only puzzling effect is the (almost weakly significant) apparent increase in early retirement between 2002 and 2003 for men. However, there are good reasons to believe that this effect is driven by compositional changes of the relatively small male sample.[14] We will turn to greater details now.

The simple mean estimates can be generalized in a regression framework in order to test the robustness of the results with respect to the inclusion of covariates:[15]

$$y = \beta_0 + \beta_1 P + \beta_2 T + \beta_3 P{*}T + \beta' X + u \qquad (1)$$

with y being the dependent variable (retirement or labor supply), P being an indicator for pension eligibility (as compared to the non-eligibility N), T being an indicator for the post-treatment period (i.e. the years 2004 and 2005 for UHBS as well as 2007 for ULMS) and $P{*}T$ being an interaction effect of P and T. X is a vector of individual, household and regional controls including marital status, education, tenure, health status, household size, a dummy for the presence of children up to age seventeen, the presence of a household member in invalidity status, household income of other working age adults, regional industry structure, settlement type as well as larger regional fixed effects. If the pension increase was truly exogenous and anticipated, the inclusion of covariates should lead to only modest changes of the results presented so far. General differences in retirement rates between pension eligible and non-eligible individuals are captured by β_1. For males, it compares retirement rates between workers aged 58 and 59 and workers aged 61 and 62, while it compares women aged 53 and 54 with women slightly above retirement age, 56 and 57 years old.[16] The β_2 reflects

[14] When including standard controls in the regression version of the DiD (see Table 3), the estimated effect shrinks to -0.053 (s.e. 0.066).

[15] The model of retirement is estimated with a linear probability model. As a robustness check a probit formulation of the model is applied, which yields slightly larger marginal fixed effects (Table 25). Recent advances in the econometric literature have suggested the use of bounded estimation for discrete DiD as counterfactual values might potentially become negative in the binary case (Athey and Imbens, 2006). In the current analysis, this concern is of less relevance as retirement levels of an appropriate control group are not expected to change radically over time.

[16] As the UHBS lacks information on exact birth dates, all those aged exactly the retirement age are excluded from the sample. Generally, it would be desirable to further control for individual unobserved heterogeneity in the labor supply responses of individuals. This can principally be done using the ULMS; however, the smaller sample size requires a broader choice of comparison age groups (four years). A drawback of the ULMS data is

common changes between treatment and control group over time which are independent of the scheduled policy, e.g., a rising trend in labor force participation over time. Hence, the approach relies on the assumption that there is no shock to the labor market which affects the two groups differently.[17] The coefficient of interest is the difference-in-difference estimator β_3 which reports the average treatment effect on those who are eligible for the treatment:

$$\beta_3 = \left(\bar{y}_{P,2} - \bar{y}_{P,1}\right) - \left(\bar{y}_{N,2} - \bar{y}_{N,1}\right) \qquad (2)$$

If the presence of the treatment after 2004 is associated with increased retirement rates, this coefficient should be positive and significantly different from zero. As higher benefits are paid to all claimants without means or retirement testing, the treatment effect can be interpreted as a pure income effect of the pension increase. A comprehensive way of controlling for various composition effects is by estimating equation (1) while stepwise including sets of covariates. Table 3 reports results from this DiD estimation and confirms that pension eligible individuals had higher retirement rates after the reform. While the inclusion of covariates substantially improves the fit of the regressions, the size of the coefficient of interest decreases only very modestly. Given the general improvements of the welfare situation of Ukrainian households during the 2000s, one might argue that the results are driven by welfare gains stemming from other household members. However, income sources generated by younger co-residing adults are controlled for in columns 5 and 6. Additionally, when restricting the sample to households without co-residing working age adults the findings are qualitatively the same.[18] The inclusion of health controls in column 4 also clearly indicates that the observed retirement effect is not driven by a general deteriorating health situation of the population, although Ukraine has indeed experienced a severe health crisis during the transition process.

The bottom panel of the regression table replicates the control experiment 2 for men and women under the stepwise inclusion rule for covariates. As before, no indication for a structural change between 2002 and 2003 is found. The initially suspicious coefficient for men drops considerably and remains insignificant as briefly discussed above.

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the gap in the observation period. The first post-reform observation is in 2007 and thus already two and a half years after the reforms took place. On the one hand this gives us the opportunity to test whether the measured effects have some persistence; on the other hand, it becomes harder to interpret the size of the treatment effects.

[17] There is no evidence, that the implementation of the pension rise was financed through rising income taxes in the short run, which could potentially affect labor supply behavior of working age persons.

[18] Results are available from the author upon request.

The empirical strategy rests on the assumption that the comparison between retirement rates of those immediately below retirement age over time is a suitable counterfactual for the treatment group. There are a couple of reasons to believe that this assumption is true for the Ukrainian case, although the assumption itself remains untestable. As pension ages are rather low in Ukraine, it seems reasonable to compare individuals shortly before and shortly after reaching the retirement age threshold without the risk of comparing adults of different physical ability to work. The two groups also show little differences in most observable characteristics except for those that are directly related to age (age, years of work experience, widowhood) (Table 4), however, one might fear that unobservable characteristics differ. The main concern stems from the substantial educational expansion that took place in the Soviet Union between 1958 and 1961 and which aimed at providing every Soviet citizen with at least a basic secondary degree. The male cohorts analyzed in this paper were affected by this expansion and a rising share of secondary educational degrees can be detected among men between the years 2003 and 2005 (see Appendix).[19] As better educated individuals retire later in Ukraine—a finding consistent across data sets and waves—the compositional change directly impacts retirement rates. Controlling for educational attainments does not convincingly solve this problem as the within comparison will provide a misleading picture; it cannot be ruled out that some highly able youth were left without secondary degree in earlier cohorts due to the lack of educational facilities while their younger fellows were better educated. However, the potential bias introduced by the educational expansion will lead to underestimating the retirement effect of the pension increase as better educated younger cohorts should exhibit retirement rates that are lower than they would have been under the same educational composition as slightly older cohorts. Consequently, estimates for men are expected to be downward biased.

Table 5 gives further insights into the nature of the pension increase by comparing several subgroups. The table investigates three hypotheses: First, we are interested into whether women respond differently to the reform than men. As mentioned before, women retire slower than men (a setting that is quite unusual for most countries of the world but probably related to the especially severe health crisis of men), but given their relatively lower labor incomes they might incure stronger retirement incentives from the equalizing pension increase. The first two columns replicate the basic result for men and women. The bottom line reports the F statistics of a Chow test and clearly rejects the equality of the coefficients.

[19] Women in these affected cohorts were already older than the treatment group.

Second, one can use the exogenous pension increase to study the relationship between health status and retirement. By definition, individuals with health conditions that result in the inability to perform work are excluded from the current analysis (by excluding individuals with disability status). The question remains whether those with reduced working capacities differ in their response to the pension increase from those without any impediments. Research investigating the impact of health status on retirement is complicated by reporting bias and the potential endogeneity of health status. Health at older ages is—among other determinants—a consequence of individual decisions taken throughout life. Empirical evidence suggests that chronically ill persons retire earlier as a consequence of lower labor market returns and higher disutility from working (Currie and Madrian, 1999). Given that chronically ill persons will be more likely to retire early, they should be less responsive to retirement incentives at older ages. As columns (3) and (4) suggest, this is the case. Upon reaching retirement age, more than 80 percent of the chronically ill are already out of the labor force and the treatment coefficient remains insignificant. Despite the small sample size, the Chow test again rejects the equality of the coefficients. This analysis suggests that the measurement of the pension income effect at normal retirement age has little explanatory power. Thus, in column (5) we test whether chronically ill people react at the minimum service year threshold for early retirement (women at 20 years, men at 25 years). Therefore, interactions between dummies indicating service time above the minimum threshold, chronic disease and the post-reform period are included in a pooled regression. The coefficient of interest is the triple interaction between the three dummies which reports that the reponse to reaching the minimum threshold as a chronically ill person after the pension increase equals 19 percentage points of additional retirement.

Finally, poorer regions should benefit stronger from the pension increase since the pension increase leveled (the modest) regional variation in pension benefits that existed until 2003. Due to the substantial geographic variation in Ukraine’s economic structure as well as wage and pension levels, a regional comparison is useful. After the pension increase, a flat benefit rate applied for virtually every pensioner thus producing variation in the magnitude of the pension gain. Columns (6) and (7) confirm that the retirement effect from the pension increase was stronger in regions which had an above median pension level growth between 2003 and 2005 and the difference between the two coefficients is significant. The last two columns of the table compare urban and rural residents and find again statistically significant different results. However, differences between the urban and rural population can be entirely explained by composition effects: when adding the full set of controls, the coefficients converge closely to 0.119 for urban residents and 0.124 for rural residents, respectively (results not shown).

As the proposed difference-in-differences approach compares persons close to the pension threshold, the estimates will be sensitive to any changes occurring among those below pension age. If early retirement incentives were reduced simultaneously with the rise in pension benefits, the findings could simply reflect a change in early retirement behavior or in occupational early retirement rules (e.g. for cost reasons). Early retirement is of some importance in Ukraine, as workers in hazardous occupations (e.g. miners) have been entitled to earlier retirement since Soviet times. The empirical evidence on the extent of early retirement remains, however, scant. Luckily, the ULMS allows shedding some light on the issue, as all job changes and job quits are recorded retrospectively from 1986 onwards. Of the entire 2003 sample, 18.9 percent (1,633 in total) retired between 1986 and 2003 and of those 8.0 percent retired through an early retirement scheme.[20] However, these numbers mask some variation over time: While early retirement schemes were quite common at the end of the Soviet period (14 percent of all retirees in 1986), labor market exits through early retirement were substantially reduced during the 1990s. During the period under consideration here (2003 to 2005), early retirement exits accounted for five to six percent of total retirement exits. However, respondents from hazardous occupations might not consider their retirement *early* if the normal retirement age in these occupations was below the statutory retirement age. Therefore, an indicator for those claiming to retire regularly but below the national normal retirement age is constructed. It turns out that the share of those in early normal retirement is slightly above 20 percent of all retirees per year and this value is virtually unchanged since 1996.[21] Early retirement is common in few occupations, especially mining. The mining sector is geographically concentrated in Ukraine (e.g., in the Donetsk and Lugansk region), however, excluding the respective regions from the analysis does not change any of the presented results (see Table 6, columns 1 and 2).

The remainder of Table 6 lends more robustness to the retirement results from an opportunity cost perspective. For their retirement choice, individuals will compare their potential pension benefits with their forgone earnings. Columns (3) to (6) thus control for a "shadow wage" in the form of potential yearly earnings. As the data set provides earnings information obviously only for those who actually work, one has to predict the shadow wage

[20] Early retirement is self-reported and coded from a multiple answer question. To check consistency of the responses, the answers were compared with the computed individual age at retirement.
[21] In the early 1990s, shares were substantially higher.

from earnings regressions that account for selectivity into employment.[22] Given the restricted information available for both workers and pensioners, the shadow wage is computed for all gender-age-education-region cells. As evident from the table, the inclusion of the shadow wage changes the treatment effect only negligibly. Some change in coefficients appears as long as covariates are uncontrolled for, which is directly linked to the way in which the shadow wage is computed. Including the shadow wage into the regression is comparable to directly controlling for its determinants. In this "raw" version, the coefficient on foregone earnings is negative, indicating that a higher earnings potential discourages immediate retirement. Additionally, this negative coefficient picks up the retirement discouraging effect from the general increase in wages in Ukraine during the relevant period. While higher labor force participation was previously directly reflected in the negative coefficient on the post-reform dummy, its sign turns after controlling for potential earnings.

A potential threat to the validity of the DiD estimates comes from household composition, which is potentially responsive to the availability of household resources (Edmonds et al., 2005; Engelhardt et al., 2005). Under the assumption that household members pool their resources, changes in their relative contribution might introduce incentives to split or unite households. To test for endogeneity in household composition, a model similar to (1) is estimated with household size as dependent variable. If households were significantly larger or smaller after the reform, we could not reject the hypothesis that household composition is responsible for the observed welfare and labor supply patterns. For different measures of household composition, the "treatment" effect from the pension increase is, however, zero (Table 7). In the ULMS data, one can make use of its panel component and detailed household roster; restricting the ULMS analysis to households that have not experienced a change in composition after the reform year 2004—except for status changes of members who "grew" into retirement age—should provide clean results. As Table 8 shows, very similar results are found when excluding those rearrangements, so that endogenous household formation can safely be ruled out as explaining factor for the observed patterns of reduced work among the pension aged.

Closely connected to the household size decision, is the fact that partners might take joint retirement decisions. From a theoretical perspective, partners wish to customise retirement dates for several reasons like complementarities in their utility functions, shared tastes as a result of assertive mating or similar economic environment and wealth (Hurd, 1990). As Ukraine has a high rate of female labor force participation, joint retirement

[22] Using the Heckit approach and pension age as exclusion restriction.

decisions will also play a role in this context. Tables 9 and 10 report some descriptive results on the joint retirement decision of couples. If anything, it seems that joint retirement increased within the joint retirement frontier (the shaded area of Table 10) suggesting that the additional income allows couples to synchronise retirement where is was earlier not feasible.

The presented DiD estimates are sensitive to a number of methodological issues, among them the choice of the width of the comparison groups around the retirement age. Table 11 reports results for a wide range of bandwidth choices. The values in column 2 simply replicate the earlier results. As evidenced in the table, the treatment effect decreases as we use broader comparison groups. This seems reasonable as we include ever-older age groups in our data aggregate which have already higher pre-reform retirement rates. In other words, the absolute additional retirement effect of the pension increase decreases with age as already evidenced graphically in Figure 4 and 5. The fact that the basic results and the precision of the estimates are preserved in a wide range of settings confirms the robustness of the outcomes.

A threat to the validity of our results from the UHBS data can potentially stem from the fact that the sample does not observe the same people over time. In order to show that the negative labor supply effect was truly induced by the pension increase, the retirement rates of those slightly above retirement age should change over time, while those of the slightly younger control groups should remain unchanged. Figure 6 shows that the labor supply of those below retirement age remained roughly constant over the four years between 2002 and 2006.[23] Quite differently, the share of retirees (up to two years above the statutory retirement age) increased between 2003 and 2005 by a fraction comparable to the DiD estimators. More formally, Table 12 compares retirement rates to the base year 2002 and tests for statistical significant differences. For the control groups below retirement age, the T-statistics remain well below two, while retirement rates for the treatment groups are significantly different from the base year in 2005 and 2006 (and 2004 for women only) as indicated by the large T values. As there were no others policies in place which could favour retirement of those close to the pension age, the reduced labor supply can be causally attributed to the increase in the legal minimum pension guarantee.

Throughout the analysis of this paper, age eligibility is used as an instrument for actual pension benefit receipt. Using the pension increase interaction as an instrumental variable in a two-stage estimation, one can gain insights beyond the reduced form estimation performed so far. Employing an activity-based definition of retirement (i.e., a dummy variable for not

[23] Again, the reform year 2004 was excluded.

working), a naive estimation of the effect of pension receipt on the participation decision is performed in Table 13. The benefit effect is 41 percentage points for men and 36 percentage points for women. When instrumenting pension benefit receipt by the interaction of retirement age and the post-reform dummy, the coefficient of pension receipt increases substantially to 64 percentage points for men (plus 56 percent) and 44 percentage points for women (plus 22 percent).[24] The first stage regression suggests a strong single instrument, while using more interactions as instruments is not advisable as the overidentification test (Hansen J statistic) rejects validity of additional instruments (not reported).[25] These results are suggestive for measurement error in benefit receipt.

At the mean retirement rate the retirement elasticity $(\partial R/\partial B)(B/R)$ with respect to benefit income ranges from 0.32 (when using the benefit elegibility rule in a probit specification) to 0.66 (when using real benefit receipt). The former is substantially below retirement or labor supply elasticites reported elsewhere in the retirement literature, while the latter falls between estimates from the 1960s/70s in the US (Krueger and Pischke, 1992) and the early 20th century US (Costa, 1995) or Brazil (de Carvalho Filho, 2008). Consistent with the previous literature, retirement seems rather inelastic with respect to income shocks.

Regression Discontinuity Estimation

Although we used relatively narrow treatment and control groups, DiD results might be biased for functional form reasons. Taking a closer look at the graphs in Figures 4 and 5 for both men and women pre- and post-reform gives four insights: First, between 2003 and 2005 the discontinuity in labor force participation at the retirement age threshold widened substantially. Second, there are no other structural breaks in the data series. As described in the Appendix, estimation density on the left side of the threshold is relatively low for men in 2003, explaining the less smooth behaviour of the scatter points below retirement age in 2003.[26] Third, the picture for women is very similar in both data sets (for men, the sample size is too small for producing sensible RD estimates using the ULMS data). This is reassuring as we observe the same people in the ULMS data, while cohorts change in the cross-sectional UHBS. Fourth, one can derive insights into the functional form around the discontinuity: In

[24] Given the high level of benefit compression there is too little variation to be exploited in benefit level regressions. Therefore, only a dummy variable taking on the value of one if a person receives an old-age pension is used here.
[25] Given the small smaple sizes, the use of several instruments would also result in lower efficiency.
[26] Using pooled samples of 2002 and 2003 data vs. 2005 and 2006 data, respectively, produces smoother trends. The evolving discontinuity over time remains qualitatively the same.

2003 we observe convexity below and concavity above the retirement threshold—the typical pattern for a gradual transition of elderly into retirement. Even without any pension system in place, we would expect an increasing number of older people exiting the labor force as their physical ability decreases etc. The pension increase in 2004 not only introduced a wider discontinuity, also the speed of retirement at points further away from the threshold has changed. For women, the pre-retirement age path becomes linear and slightly flatter. After the reform, linearity appears to the right of the threshold for men, while the labor force participation function for women remains concave. The change in functional form might bias DiD results reported earlier.

Moving from the DiD to an RD design might improve our estimates, as we allow for more flexibility in functional form around the threshold. Also, using more data points might add to estimation precision. Upon reaching retirement age, the probability of receiving an old-age pension (i.e. the binary treatment) jumps discontinuously. The discontinuity used here to identify the income effect in the retirement decision is based on an eligibility criterion defined by age. Regression discontinuities in age eligibility generally differ from ordinary RD designs in that individuals cannot reject the assignment to treatment and in that the assignment to treatment is certain (Lee and Lemieux 2009).[27] The basic idea of the sharp RD design is that the causal treatment effect of the model $y_i = \alpha_i + x_i \beta_i$ can be obtained by comparing mean outcomes of those aged slightly above with those slightly below the treatment threshold:[28]

$$\beta = y^{+} - y^{-} \tag{3}$$

In order to estimate the income effect from the pension increase over time, a combination of two regression discontinuity estimators generates the Regression Discontinuity Difference (RDD) estimator. Using a parametric version of the RD design implemented by lower-order polynomial regressions, one can estimate the change in the retirement ratio at the retirement age between the pre- and post-reform year:[29]

$$ATE = E\left[Y_{2005i}(1) - Y_{2005i}(0) \middle| X = c\right] - E\left[Y_{2003i}(1) - Y_{2003i}(0) \middle| X = c\right] \tag{4}$$

[27] The basic mechanism and identifying conditions of RD designs are laid out in greater detail in Hahn, Todd and van der Klaauw (2001).

[28] The absence of exact date of birth information in UHBS forces me to implement the regression discontinuity estimator with relatively broad discrete categories (years of age). Producing evidence form “narrower” discrete age variables would be desirable but introduce small sample problems.

[29] In the estimation polynomials of degree two are applied. The age variable is centred at the gender-specific retirement age. The results are robust to the use of higher order polynomials.

As noted in Lee and Lemieux (2009), the validity of the RD design can be checked by including covariates, which should neither change the model estimates of interest nor their standard errors.

The results from the visual inspection are confirmed by the various RD regression estimates. Table 14 shows that the retirement effect of the pension increase for men is significantly positive and very stable when adding covariates in a stepwise fashion. Thus, the data confirm the theoretical irrelevance of covariates for the pure income effect (cp. Lee and Lemieux, 2009). Also for women, the RDD estimates confirm earlier findings, although estimates are somewhat smaller. All in all, the RDD estimates compare quite well to the DiD results.

Intensive margin of labor supply

The research on retirement decisions distinguishes between labor supply responses at the extensive vs. intensive margin. In the latter case, persons retire gradually and reduce the number of working hours or working weeks rather than fully retreating from the labor market.

The ULMS is a useful data source to uncover changes at the intensive margin, as it offers a variety of information on normal and actual working hours during the reference week, weeks worked per year as well as information about deviations from the contractual work load.[30] As the ULMS is a longitudinal data set, the results are not affected by changing educational quality of treatment and control group across years as it is possible to control for unobserved heterogeneity. The following analysis is based on three main outcomes, yearly working hours, weeks worked per year and weekly working hours. The results suggest some strong effects for women and those with low education.

As briefly mentioned above, labor relations are strongly regulated by the state and the Soviet Labor Code which is in force since June 1972 prescribes the average working of 40 hours. Again, regulated exemptions apply in hazardous occupations and, for instance, for teachers. Enterprises do generally not seek to allow for more flexibility in working time rules, as overtime work must be paid twofold. Part-time work was very untypical during Soviet times and employment with reduced working hours is only emerging slowly.[31] As a response,

[30] For ULMS results on the retirement decision see Table 8.
[31] The questionnaire layout of the ULMS accounts for this peculiarity. Individuals are asked for their normal working hours and whether they normally work 40 hours; if not, respondents can chose from a list of reasons, most of which are related to exogneous shocks, like "shortage in work material" or „sickness". Almost half of those who work less than fourty hours per week report that the shorter working time is considered full-time in

working time is more often adjusted through weeks per year rather than hours per week. Still, the share of workers who reduced work load rather than fully retired is surprisingly low, and the vast majority is concentrated in low skilled service sector occupations (with teachers being the only numerous exception).

As Table 15 shows, women reduce their yearly labor supply of hours by 281 hours or on average seventeen percent. However, the effect is strongest for least educated women and also applies for least educated men. Workers in the lowest educational group (primary or unfinished secondary education) reduce their yearly work load by 460 hours, which is a substantial minus of 34 percent. These results are confirmed in the regression set-up (Table 16) and robust to the stepwise inclusion of various control variables (Tables 17 and 18) as well as controlling for unobserved heterogeneity by individual fixed effects (Table 19). In boh samples, the Hausman test suggests preference of the random effects model over the fixed effects model on efficiency grounds. The coefficient from the random effects estimation is slightly less precisely estimated, but even larger for those with low education.

The results show two interesting insights: First, labor supply adjustments at the intensive margin are predominantly realized through the number of working weeks rather than the number of weekly working hours. As Tables 15, 17, 18 and 20 show, working hours change relatively little. This suggests that workers adjust labor supply differently when they are strongly constrained in their hours' choice set as is the case in Ukraine. Second, there are no labor supply effects at the intensive margin for the male sample which probably relates to the gender specific occupational structure in Ukraine. Labor relations in most jobs are strictly regulated and reduced working hours are only possible in few (with the exception of teachers mostly low skilled) service occupations. Women who reduced their yearly working time by at least ten percent of weeks are teaching professionals or employed in elementary service and sales occupations. Male teaching professionals, drivers, mobile plant operators as well as craft and trade operators were most likely to reduce working weeks by more than ten percent. Similarly, among those women who reduced their weekly working hours by at least 25 percent are predominantly teaching professionals, sales personnel and elementary service and sales occupations. Men who work as drivers, mobile plant operators, craft and trade operators as well as in elementary service and sales occupations were most likely to reduce their working hours.

One concern could be that the retirement choice is partly correlated with the much earlier occupational choice. If some individuals chose a specific occupation also for better

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their occupation (e.g., teachers). Only 15 percent of respondents say that they deliberately want to work less than 40 hours per week, and this share has not changed between 2003 and 2007.

prospects of early or late retirement, ignoring the occupational choice my lead to biased estimation. The ULMS luckily offers a comprehensive retrospective labor market history until the year 1986, from which we can infer occupational choices. When controlling for the occupation held in 1986 (which can be considered exogenous to retirement decisions taken between 2003 and 2007) the results remain robust (Table 20). Other robustness checks in this table include the exclusion of individuals who live in households that changed their composition between 2003 and 2007 and the sample split in workers that report to suffer from at least one out of seven chronic diseases. Chronically ill women reduce their yearly labor supply substantially, while we find little evidence for other adjustments.

Retirement incentives across the educational distribution

The generous pension increase depicted in Figure 1 and the high level of benefit compression suggest that retirement incentives should be higher for low income earners, who gain disproportionally from the equalizing benefit rate (also Noel et al., 2006). At closer inspection, however, two opposing effects determine the relative retirement incentives. While higher income levels are associated with higher marginal cost of giving up additional income (implying that high income earners are relatively less likely to retire), they are also associated with lower marginal utility of income (implying that high income earners are relatively more likely to retire). In total, the effect is theoretically ambiguous.

Let us consider the retirement decision as a discrete choice at every point in time; the economic rationale whether or not to retreat from the labor market depends on the comparison of costs and benefits of prospective lifetime income flows under different retirement regimes (cp. Belloni and Alessie, 2009). From an actuarial perspective, there exists one (or several) optimal point(s) in time at which the income flow will be maximized (cp. Stock and Wise, 1991). Instead of picking the individual optimal retirement date, the interest here is on a static comparison of retirement choices before and after the minimum pension increase. Net present values of lifetime income that representative individuals would face upon reaching the retirement age can be calculated from UHBS data. The lifetime wealth at normal retirement age t can be computed as the sum of the social security wealth and the wealth from working over the retirement age:

$$\text{NPV} = \sum_{s=t}^{T} \pi(s) \frac{B(t)}{(1+\delta)^{(s-t)}} + \sum_{r=t}^{R} \pi(s) \frac{Y\,(t)}{(1+\delta)^{(r-t)}}$$

where an individual can chose to keep on working and earn a yearly income Y in addition to the yearly pension benefits B up to the real retirement age R, after which B is the sole source of income. As the Ukraine is characterized by a high degree of benefit compression and therefore an extremely low correlation between lifetime earnings and pension benefits, B can actually be treated as a constant. The probability to survive until period s is indicated by $\pi(s)$.[32] Assume that a person reaching pension age has to decide whether to keep on working or to retire immediately. For this decision, the entire lifelong wealth accumulation is relevant. To show the incentive structure in the Ukrainian case, two scenarios are presented, one in which the individual retires immediately upon reaching the retirement age ($R=0$ and $s=t$) and one in which the individual works three more years before retiring. Table 21 compares the lifetime wealth for three broad educational groups of men and women in the respective scenarios and reports the cost attached to immediate retirement. For both sexes, the results for 2003 show substantial variation between educational groups with better educated individuals incurring higher costs for immediate retirement, up to 37 percent. Given the substantial pension compression which can be directly seen when comparing the absolute NPVs in the "immediate retirement" rows, this is not surprising. Looking at the wealth levels for 2005, one can observe a general welfare improvement. The overall cost pattern remains the same (better educated incurring higher costs), however, the reduction in the retirement penalty is disproportionally larger for the lower educational group. The pension reform reduces the cost of immediate retirement for a low educated worker by 35 percent, while the retirement penalty for better educated falls by only one fifth.

Figure 7 confirms that the stronger actuarial retirement incentives translate into stronger retirement responses among the less educated. The downward sloping line links the levels of treatment effects across the educational distribution. Treatment effects of the Cumulative Density Function (CDF) are interaction dummies between levels of education (measured in years of schooling) with the treatment indicator. Up to 14 years of schooling, the pension increase induced additional retirement, while no impact can be detected for the best educated. Table 22 reports standard errors for the estimates presented in the Figure, confirming that there is no statistical retirement effect above 13 years of schooling. The group of those with nine years of schooling is small in size, leading to imprecise estimation.

[32] To compute the NPV, one has to make assumptions about life expectation at retirement age and about time preferences (discount rates δ). Life expectancy values at retirement age are taken from Gora (2008); for the discount rate three percent is assumed (as we are comparing very narrowly defined scenarios here, the simulations are not very sensitive to the choice of the discount rate). For computational details see the Note of Table 21.

Additional support comes from the direct investigation of individual monetary gains from the reform. Based on various individual characteristics it is possible to predict the potential pension benefit that each individual could expect before and after the reform. Comparing these two simulated pension values, one can construct the potential pension growth that varies with gender, education and region (as a proxy for industry structure). As expected during pension compression, persons with higher pension entitlement in 2003 experienced below average potential pension growth—the correlation coefficient between actual pension benefits in 2003 and potential pension growth is -0.39. When splitting the sample at the median pension growth, it turns out that those individuals that could expect higher pension rises indeed show stronger retirement responses (Table 23, columns (1) and (2)). Adding the potential pension growth as covariate in the basic DiD framework does not change any of the previous results, while the coefficient for potential pension growth is significantly positive (Table 23, columns (3) and (4)).

4. Pension income increase and old-age poverty reduction

The public policy objective of the pension increase was to reduce old-age poverty. This said, one has to acknowledge that poverty is a multi-facetted concept in itself and that the measurement of poverty is non-trivial. As such, only selected evidence of the poverty-reducing effect of the minimum pension rise will be presented. The poverty reducing effect can be measured straightforward in income terms.[33] When combining all individual yearly income sources (including net labor income, state transfers, gross transfers, interest and dividends from the individual questionnaire), it is possible to determine whether a person earned sufficient funds to autonomously surpass an absolute poverty line, which is defined as the 2.15 USD poverty line used by the World Bank. Figure 8 shows that prior to the reform year, the share of those above the poverty line was generally low, but lower for those in retirement age. Among the retirees, poverty was positively correlated with age. The right panels of the Figure show, first, that the ratio of those below the poverty line had shrunk dramatically until 2005. This effect is due to the substantial growth experienced by Ukraine during the 2000s. Second, elderly people are by 2005 less likely to be poor when compared to the working aged. The substantial improvement of the welfare at older ages has also

[33] Although it might be preferable to measure poverty in terms of consumption, substantial difficulties stem from the pooling of household resources and the lack of individual level consumption data (for a comparison of income and consumption poverty in Ukraine, see Brück, Danzer et al., forthcoming).

eradicated the disadvantaged welfare situation of very old retirees. Table 24 indicates that the poverty reduction effect which can be attributed to the minimum pension rise was between 16 and 23 percentage points for the pooled sample, respectively. Also in relative terms, pensioners advanced with respect to the mean of disposable income in the 45 to 65 year old population by 19 to 24 percent. The absolute and relative improvement of the economic situation of pensioners confirm the graphical evidence from Figure 8. To sum up, the government's pension increase has significantly improved pensioners' economic position as formulated in the general policy objective of the minimum pension increase.

5. Conclusions and policy implications

This paper provided econometric evidence that a substantial minimum pension increases like the one implemented in Ukraine in 2004, has the potential to lift pensioners out of poverty. At the same time, the reform significantly reduced the labor supply of both, pension eligible men and women after reaching retirement age. These labor supply adjustments reflect an increased probability of retirement between 30 and 47 percent. Most likely, those behavioural responses have reduced the pure welfare effect of the pension increase.

On the aggregate level, the reduction of the labor force is of non-negligible size. When computing induced retirement for the first three post-retirement age cohorts, the workforce shrinks by 94,000 men and 158,000 women. The overall effect of the pension increase can be expected to amount to roughly 413,000 persons or 2.4 percent of the pre-reform labor force. Unlike in industrialized countries the relatively static nature of the Ukrainian labor market allowed only modest adjustments of individual labor supply at the intensive margin due to the absolute predominance of full-time contracts with inflexible hours. Yearly work load reductions were predominatly realized by women and low educated service sector workers through adjustments in yearly working weeks.

The natural experiment of the pension increase in Ukraine allows drawing some general conclusions for developing and transition countries. For formerly Socialist countries the Ukrainian case is insightful, as they share a common Socialist labor market legacy and similarly structured pension systems, including relatively good coverage, low retirement ages as well as low correlation between contributions and benefits.[34] This study provided robust evidence in a developing context indicating that

- an increase in pension income reduces labor supply at older ages for men and women,
- the use of a minimum pension guarantee or a flat pension benefit might install strong labor market incentives that will differ for various subgroups of the labour force,
- a generous full-coverage pension system is able to achieve welfare objectives (reduce old-age poverty), although the success of such a systems has to be contrasted with its labor supply effects, fiscal costs and the intergenerational burden. The results from the analysis suggest that well-informed public welfare policy should take into consideration potential effects on individual labor supply. The policy goal to combat poverty via pension increases might become ineffective and fiscally extremely costly, when the pension aged withdraw their manpower from the labor market. As a consequence, overall welfare levels might increase less than in a static framework without labor supply response.

As argued above, the estimated treatment effects have the interpretation of causal pure income effects due to the non-means-tested and non-retirement-tested nature of the Ukrainian pension system. As such, a note on the external validity of the results is warranted. The presented estimates and elasticities are in line with the previous literature and thus confirm the existence of retirement responses to positive income shocks in a developing setting; retirement seems, in general, rather inelastic. All results are short-run responses to an unanticipated pension increase and differ from unanticipated social security rises in the US (Moffitt, 1987) by their sheer magnitude and by institutions that promoted myopia among agents.[35] As the analysed pension increase exacerbated the fiscal stress of the pension system and as labor force participation is hard to forecast in a highly dynamic environment, it is an open question whether the effects of the pension increase will remain significant in the future.

[34] While most formerly Socialist Middle European countries have already implemented full pension reforms, most Eastern European countries are still awaiting the advent of the changes in the pension system.

[35] During the transition process, most state institutions became unreliable in the eyes of the population which was so used to full state provision of social services. As such, life-cycle maximizing behavior of the population seems rather unlikely.

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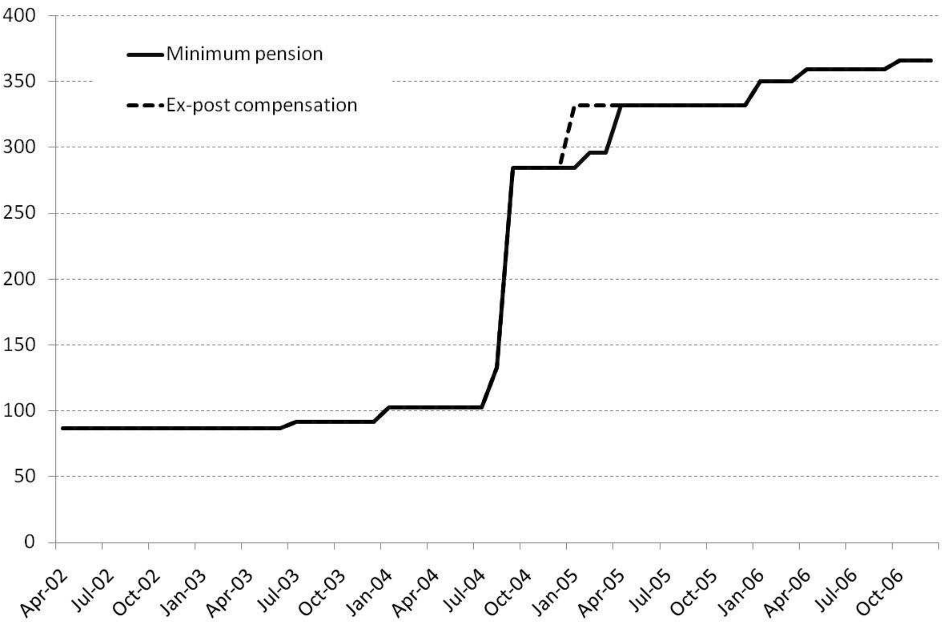
Figures and Tables

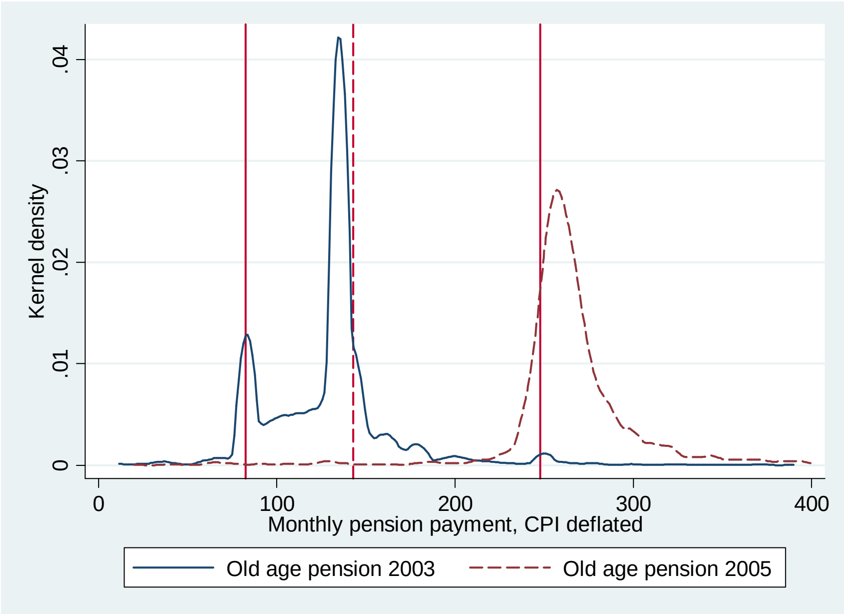
Figure 1

Development of legal monthly minimum pension over time (in UAH)



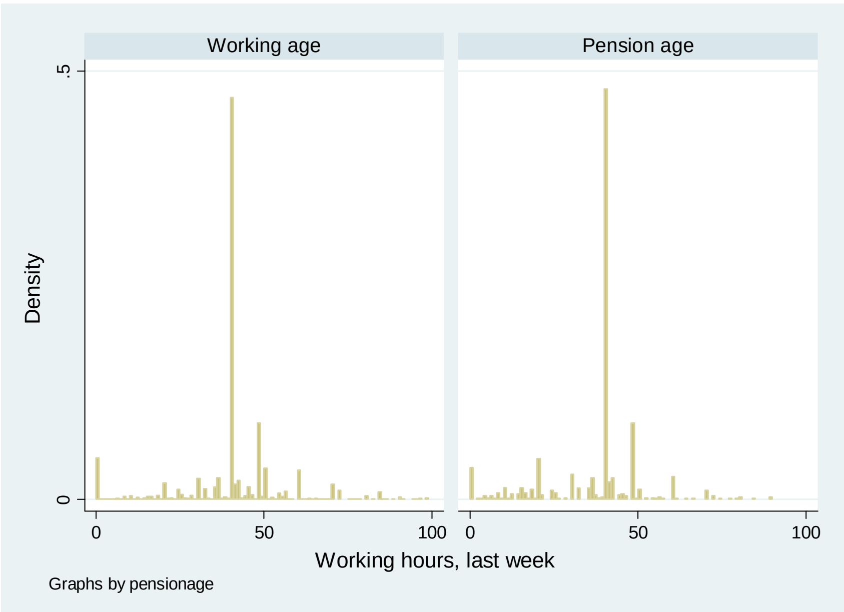
Note: The reported values are in nominal terms in Ukrainian Hryvnia (UAH). In September 2004, the Cabinet of Ministers decided to raise the legal minimum pension guarantee to the subsistence minimum. Between January and March 2005 the pension level did not change much (black line), but in April 2005 the government compensated pensioners ex-post to reach a higher benefit level (dashed line). It was only in April 2005 that the government also amended the State Budget Law and implemented the new Pension Law which codified the higher pension rights. Source: Cabinet of Ministers, Ukraine.

Figure 2
Distribution of average monthly pension payments (in UAH), change 2003 to 2005



Note: The superimposed full vertical lines mark the average monthly legal minimum pension for 2003 (left) and 2005 (right). The monthly legal minimum standard is computed as weighted average about the preceding 12 months. In 2005, the legal minimum pension rose slightly between January and April, however, pensioners were ex-post compensated by the government, so that the nominal pension level was 332 for all months in 2005. The dashed vertical line marks the state pension cap which was in place prior to the reform. Pension incomes are deflated by national CPI to December 2002. Source: UHBS; author's calculations.

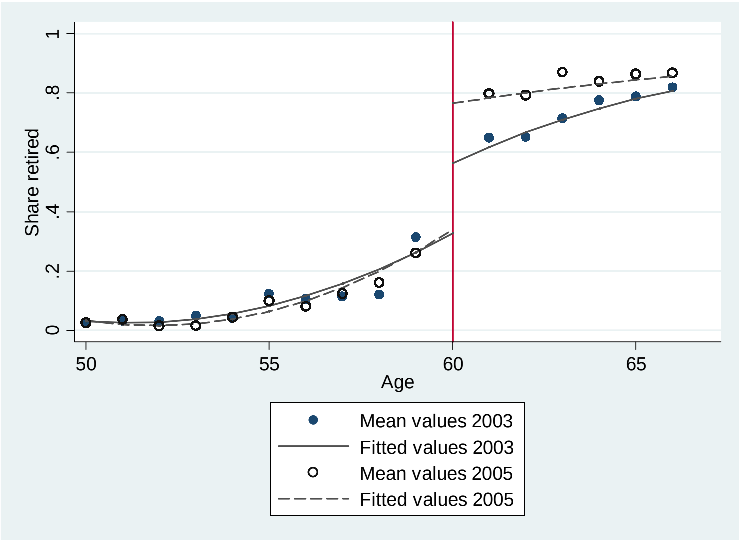
Figure 3
Working hours of working age vs. pension age individuals (actual working hours)



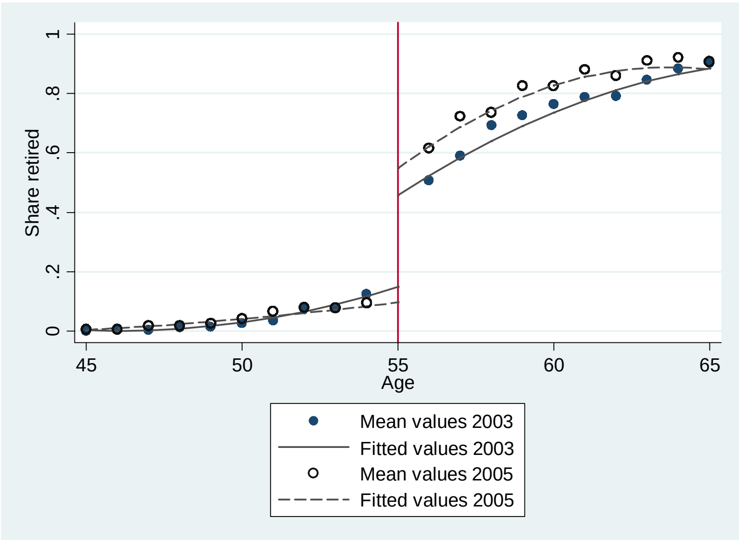
Source: ULMS; author's calculations.

Figure 4
Retirement rates across age and years

Share of retired men in 2003 and 2005

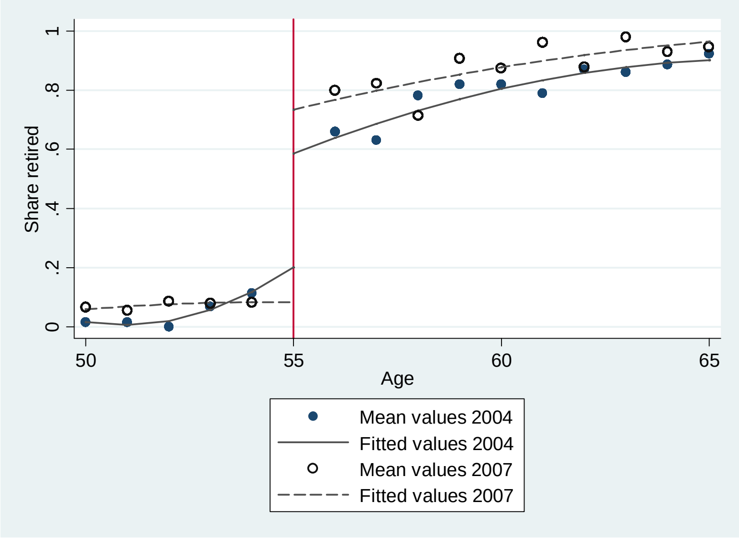


Share of retired women in 2003 and 2005



Note: Fitted values are predictions from weighted polynomial regressions (of degree two). The use of other polynomials (cubic, quartic) yields very similar results and can be obtained from the author upon request. Estimation performed for ten-year brackets at both tails. Source: UHBS data; author's calculations.

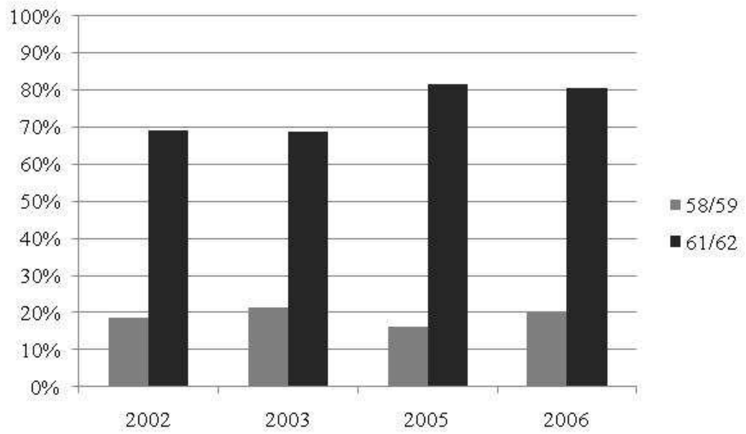
Figure 5
Share of retired women in 2004 and 2007, ULMS sample



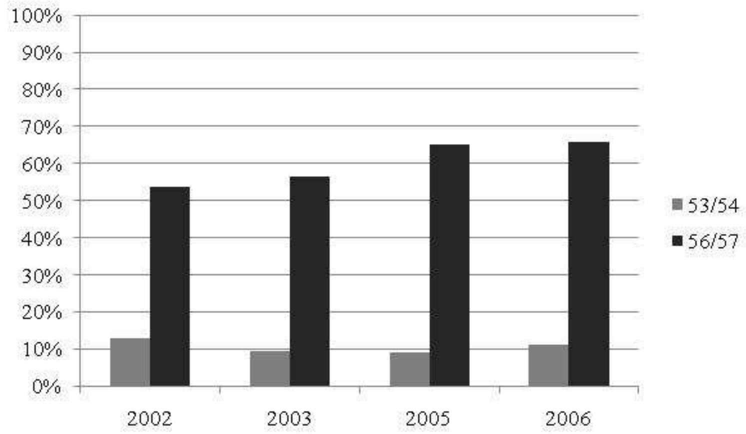
Note: Retirement is defined as receiving old-age pension benefits and reporting no income-generating activity in the reference week. Those in the retirement age directly report that they are not searching for jobs because of having reached the retirement age. Income generating activities comprise having dependent employment for at least one hour per week with the expectation to be paid (including temporary and casual work), working in a family enterprise (even when being unpaid helper) or being self-employed or entrepreneur. Income generating activities exclude pure subsistence agriculture. The definition of “income generating activity” differs slightly between the 2004 and 2007 wave of the ULMS, however, the definition chosen here guarantees the highest possible level of comparability. The labor force basis excludes individuals who are receiving disability pensions and those who have retired on early retirement schemes (retirement for years of service). Some very few individuals report being generally entitled to old-age benefits, but having recently not been paid benefits (pension arrears); those individuals are included in the pensioner group. Source: UHBS data; author’s calculations.

Figure 6
Retirement rates across survey years

Panel A. Men

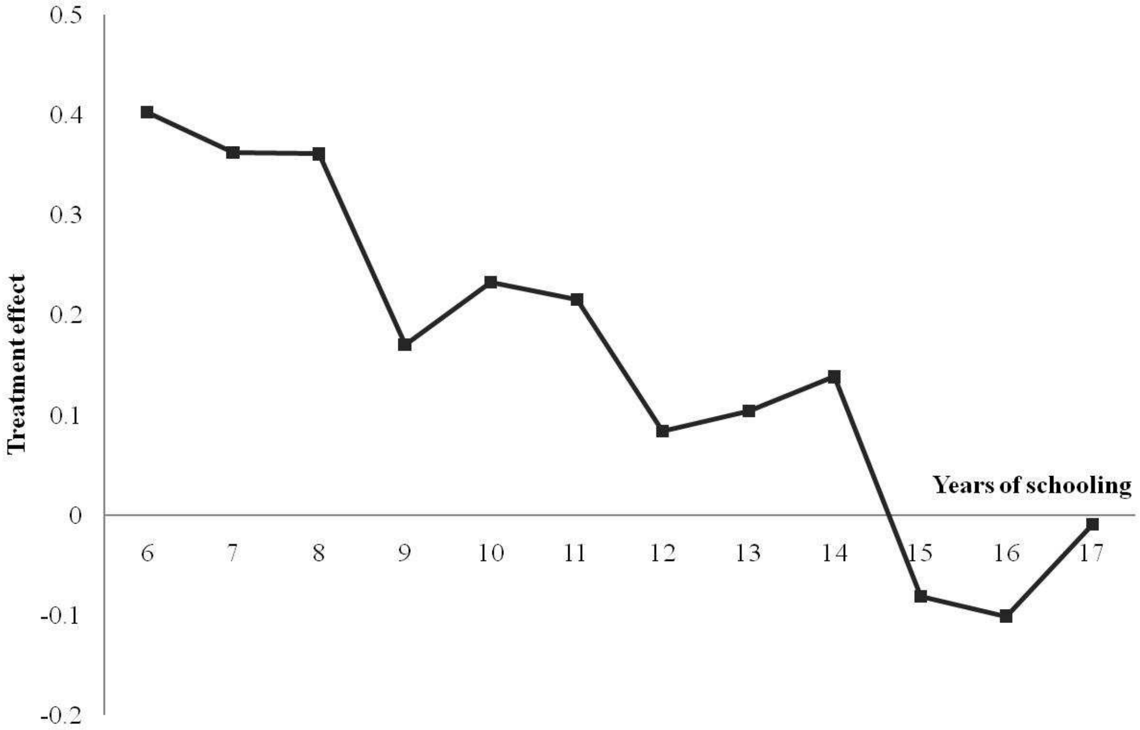


Panel B. Women



Source: UHBS; author's calculations.

Figure 7
Difference-in-Differences in educational CDF; dependent variable: retired

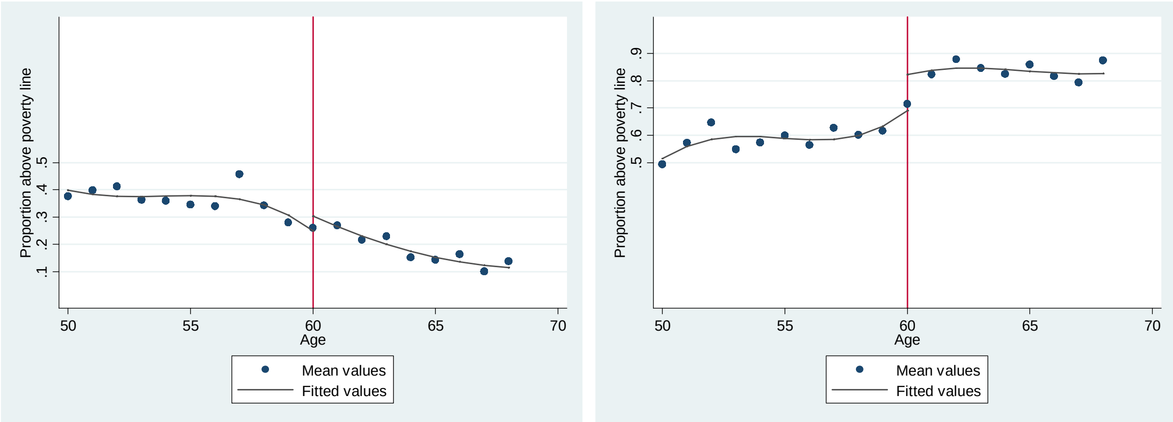


Source: UHBS; author's calculations.

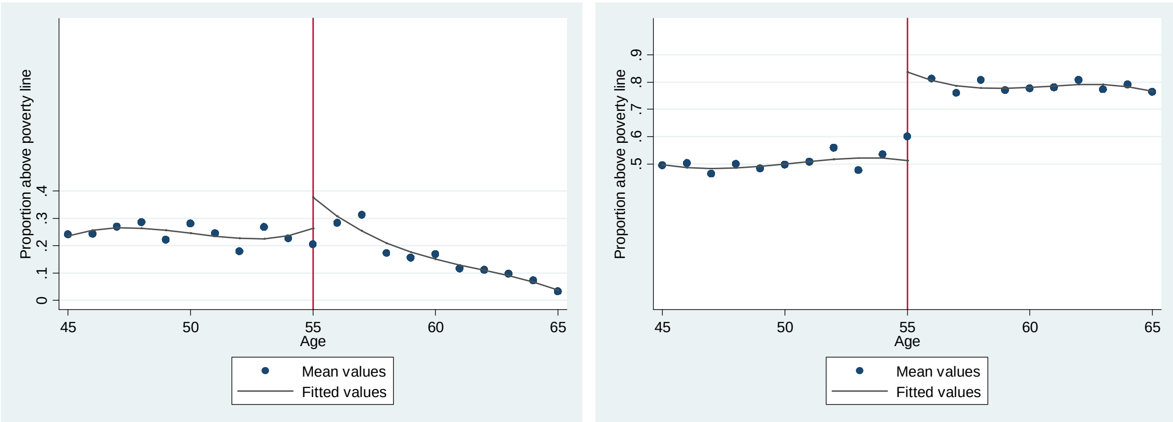
Figure 8

Poverty reducing effect of the pension increase

Proportion of men with personal income above the poverty line, 2003 (left) and 2005 (right)



Proportion of women with personal income above the poverty line, 2003 (left) and 2005 (right)



Note: Fitted values are from a quartic polynomial regression to the left and to the right of the cut-off point. Estimation performed for ten-year brackets at both tails. Poverty line is an absolute poverty line of 2.15 USD according to the World Bank. Personal income is one twelfth of the sum of all yearly income components of a person, including labor incomes (including outstanding income and inkind payments), various transfer incomes (stipends, four types of pensions, unemployment benefits), interest, dividends, revenues, and other incomes.
Source: UHBS data; author’s calculations.

Table 1
Variable description

Variable	Definition UHBS	Definition ULMS
Individual variables		
Retirement aged**	Dummy = 1 if (i) a women is at least 55 years of age or (ii) a man is at least 60 years of age	Dummy = 1 if (i) a women is at least 55 years of age or (ii) a man is at least 60 years of age
Retired	Dummy = 1 if respondent is not working, receives an old age pension and considers oneself as pensioner	Dummy = 1 if respondent is not working, not searching for a job because of “old-age retirement” and receives an old age pension
Yearly working hours	—	Number of yearly working hours in current job computed from ordinary weekly working hours and ordinary weeks worked per year
Yearly working weeks	—	Number of ordinary weeks worked per year in current job
Weekly working hours	—	Number of ordinary hours worked per week in current job
Years of schooling	Adjusted years of schooling were recalculated from information about total years of schooling and the highest educational degree ever attained	Adjusted years of schooling according to the scheme in Brück, Danzer, Muravyev, Weisshaar (2009)*
Age	Self-reported age of respondent in years	Age of respondent in years; calculated from birth information*
Married	Dummy = 1 if self-reported marital status of respondent is married	Dummy =1 if self-reported marital status of respondent is married or cohabiting
Widowed	Dummy = 1 if self-reported marital status of respondent is widowed	Dummy = 1 if self-reported marital status of respondent is widowed
Tenure	Lifetime work experience in years	Work experience in years
Health variables	Body-Mass-Index and dummy for chronic disease (respondent reports disease and negative impact on physical activity)	Dummy =1 if person reports one out of seven diagnosed chronic diseases
Household variables		
Household size	Number of persons sharing a common budget and living at the same address	Number of persons currently sharing a common budget and living at the same address
Number of working age adults	Total number of persons in working age in household; women 20-54, men 20-59	Total number of persons in working age in household; women 20-54, men 20-59
Income by the working aged	Sum of all incomes from the working aged population between 20 and 45 years in the household; including labor income, gross transfers, dividends and capital income, state benefits; calculated from individual questionnaires	Sum of all incomes from the working aged population between 20 and 45 years in the household; including labor income, gross transfers, dividends and capital income, state benefits; calculated from individual questionnaires
Invalid person in HH	Dummy = 1 if household contains a person with invalidity status	—
Children up to age 17 in HH	Dummy = 1 if household contains children up to age seventeen	Dummy = 1 if household contains children up to age seventeen
City, Town, Village	Dummies = 1 if respondent lives in urban settlement of big size, smaller size	Dummies = 1 if respondent lives in urban settlement from 100,000

	or in rural settlement	inhabitants, settlement up to 99,999 inhabitants or rural settlement
Oblast	Dummies for oblasts (26 regions)	Dummies for oblasts (26 regions)
Interview year	Dummies for all interview years 2002-2006. Interviews were taken in December.	Dummies for all interview years 2003, 2004, 2007. Interviews were predominantly taken between May and July.
Industry variables		
Regional share of employment in mining	Share of regional employment of the workforce in the mining sector, computed for 78 regional clusters	—
Regional share of employment in agriculture	Share of regional employment of the workforce in agriculture, computed for 78 regional clusters	—
Regional share of employment in state sector	Share of regional employment of the workforce in the state sector, computed for 78 regional clusters	—
Unemployment rate	Unemployment rate, computed for 78 regional clusters	—

Note: * These variables were cleaned to generate consistency across panel waves. ** For further robustness a variable was created that additionally requires a minimum of twenty years of work experience for women and twenty five years of work experience for men.

Table 2
Means of retirement rates—by age group and reform exposure; dependent variable: retired; UHBS data

Experiment of Interest: Reform year 2004, retirement age at 60 (men) and 55 (women)

Panel A. Men	2002-2003 Pre-reform	2005 Post-reform	Difference	Panel B. Women	2002-2003 Pre-reform	2005 Post-reform	Difference
Age 58-59	0.215	0.166	-0.049	Age 53-54	0.111	0.078	-0.034
	(0.027)	(0.032)	(0.042)		(0.015)	(0.015)	(0.021)
Age 61-62	0.689	0.816	0.127	Age 56-57	0.552	0.651	0.099
	(0.022)	(0.034)	(0.041)		(0.023)	(0.026)	(0.035)
Difference	0.474	0.649	0.176	Difference	0.440	0.573	0.133
N=1097	(0.035)	(0.047)	(0.059)	N=1845	(0.028)	(0.030)	(0.041)

Control experiment 1: Artificial retirement age at 58 (men) and 53 (women)

Panel A. Men	2002-2003 Pre-reform	2005 Post-reform	Difference	Panel B. Women	2002-2003 Pre-reform	2005 Post-reform	Difference
Age 57	0.171	0.159	-0.012	Age 52	0.078	0.062	-0.016
	(0.034)	(0.037)	(0.051)		(0.016)	(0.022)	(0.027)
Age 58-59	0.215	0.166	-0.049	Age 53-54	0.111	0.078	-0.034
	(0.027)	(0.032)	(0.042)		(0.015)	(0.015)	(0.021)
Difference	0.044	0.008	-0.037	Difference	0.033	0.015	-0.018
N=685	(0.044)	(0.049)	(0.066)	N=1334	(0.022)	(0.027)	(0.034)

Control experiment 2: Artifical reform between 2002 and 2003

Panel A. Men	2002 Pre-reform	2003 Post-reform	Difference	Panel B. Women	2002 Pre-reform	2003 Post-reform	Difference
Age 58-59	0.163	0.266	0.103	Age 53-54	0.129	0.094	-0.034
	(0.032)	(0.043)	(0.054)		(0.022)	(0.019)	(0.028)
Age 61-62	0.692	0.685	-0.006	Age 56-57	0.536	0.564	0.028
	(0.032)	(0.032)	(0.045)		(0.034)	(0.032)	(0.047)
Difference	0.529	0.420	-0.110	Difference	0.408	0.470	0.062
N=757	(0.045)	(0.054)	(0.070)	N=1106	(0.041)	(0.037)	(0.055)

Note: Reported values are retirement rates. Robust standard errors in parentheses. Source: UHBS; author’s calculations.

Table 3
Difference-in-Differences—stepwise inclusion of covariates; dependent variable: retired; UHBS data

Men, aged 58/59 vs. 61/62

	(1)	(2)	(3)	(4)	(5)	(6)
Experiment of interest: Treatment effect of minimum pension increase in September 2004						
Treatment effect	0.176***	0.158***	0.147**	0.143**	0.149***	0.151***
	(0.059)	(0.058)	(0.057)	(0.056)	(0.055)	(0.055)
Constant	0.215***	0.159**	-62.581*	-60.321*	-61.042*	-60.959*
	(0.027)	(0.076)	(32.042)	(31.928)	(31.779)	(31.726)
Observations	1097	1097	1097	1097	1097	1097
R-squared	0.272	0.326	0.368	0.373	0.382	0.384
Control experiment: Treatment assumed in 2003						
Treatment effect	-0.110	-0.099	-0.062	-0.061	-0.049	-0.046
	(0.070)	(0.068)	(0.066)	(0.066)	(0.066)	(0.066)
Constant	0.163***	0.116	-42.685	-41.951	-40.141	-40.293
	(0.032)	(0.095)	(40.276)	(40.346)	(40.609)	(40.924)
Observations	757	757	757	757	757	757
R-squared	0.210	0.288	0.333	0.336	0.342	0.347

Women, aged 53/54 vs. 56/57

	(1)	(2)	(3)	(4)	(5)	(6)
Experiment of interest: Treatment effect of minimum pension increase in September 2004						
Treatment effect	0.133***	0.126***	0.105***	0.105***	0.107***	0.110***
	(0.041)	(0.040)	(0.038)	(0.038)	(0.038)	(0.038)
Constant	0.111***	0.065	27.528	27.445	28.098	25.555
	(0.015)	(0.059)	(18.653)	(18.677)	(18.669)	(18.556)
Observations	1845	1845	1845	1845	1845	1845
R-squared	0.271	0.326	0.380	0.380	0.381	0.385
Control experiment: Treatment assumed in 2003						
Treatment effect	0.062	0.080	0.085	0.085	0.084	0.077
	(0.055)	(0.054)	(0.052)	(0.052)	(0.053)	(0.052)
Constant	0.129***	0.038	18.133	18.132	18.964	17.135
	(0.022)	(0.071)	(25.331)	(25.354)	(25.392)	(25.059)
Observations	1106	1106	1106	1106	1106	1106
R-squared	0.221	0.290	0.347	0.347	0.348	0.355
Region & Place FE	—	X	X	X	X	X
Individuals controls	—	—	X	X	X	X
Health controls	—	—	—	X	X	X
Household controls	—	—	—	—	X	X
Industry structure	—	—	—	—	—	X

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 4
Mean comparison—prior and after reform, control and treatment group

	Women											
	Prior to reform		**Post-reform**				**Below retirement age**		**Above retirement age**			
	Mean	**s.e.**	**Mean**	**s.e.**	**Difference**	**s.e.**	**Mean**	**s.e.**	**Mean**	**s.e.**	**Difference**	**s.e.**
Retired	0.334	(0.014)	0.409	(0.018)	0.075	(0.023)	0.100	(0.010)	0.607	(0.016)	0.506	(0.019)
Age	54.94	(0.047)	55.19	(0.056)	0.243	(0.073)	53.52	(0.017)	56.44	(0.016)	2.922	(0.023)
Married	0.655	(0.014)	0.654	(0.018)	-0.001	(0.023)	0.670	(0.016)	0.639	(0.016)	-0.031	(0.022)
Widowed	0.149	(0.011)	0.172	(0.014)	0.023	(0.018)	0.130	(0.011)	0.185	(0.013)	0.055	(0.017)
Years worked	31.52	(0.154)	31.10	(0.172)	-0.428	(0.235)	30.29	(0.152)	32.33	(0.165)	2.038	(0.226)
Years of schooling	11.79	(0.080)	12.00	(0.088)	0.208	(0.121)	11.99	(0.081)	11.77	(0.087)	-0.214	(0.119)
At least 12 yrs of schooling	0.495	(0.015)	0.574	(0.018)	0.079	(0.024)	0.541	(0.017)	0.513	(0.016)	-0.028	(0.023)
At least 14 yrs of schooling	0.233	(0.013)	0.222	(0.015)	-0.011	(0.020)	0.221	(0.014)	0.236	(0.014)	0.014	(0.020)
Household size	2.591	(0.038)	2.620	(0.047)	0.028	(0.061)	2.649	(0.042)	2.560	(0.041)	-0.089	(0.059)
Children up to 17 in household	0.213	(0.012)	0.218	(0.015)	0.004	(0.020)	0.217	(0.014)	0.214	(0.013)	-0.003	(0.019)
Person with invalidity status in household	0.056	(0.007)	0.074	(0.010)	0.018	(0.012)	0.070	(0.009)	0.057	(0.008)	-0.013	(0.011)
Total income of other household members	945.58	(64.63)	1574.04	(123.54)	628.46	(128.25)	1318.75	(98.83)	1085.10	(80.27)	-233.66	(126.48)
Body Mass Index	27.48	(0.129)	27.60	(0.148)	0.118	(0.199)	27.37	(0.141)	27.68	(0.134)	0.313	(0.195)
Reduced physical activity	0.362	(0.016)	0.307	(0.019)	-0.054	(0.025)	0.317	(0.018)	0.361	(0.017)	0.044	(0.025)
Chronic disease	0.061	(0.007)	0.055	(0.008)	-0.006	(0.011)	0.051	(0.007)	0.067	(0.008)	0.016	(0.011)
Medical treatment	0.099	(0.009)	0.106	(0.011)	0.007	(0.014)	0.095	(0.010)	0.108	(0.010)	0.012	(0.014)
Regular physical activity (sport)	0.129	(0.010)	0.111	(0.012)	-0.018	(0.016)	0.117	(0.011)	0.126	(0.011)	0.009	(0.015)
Village	0.289	(0.014)	0.348	(0.018)	0.058	(0.022)	0.292	(0.015)	0.332	(0.015)	0.039	(0.022)
Town	0.296	(0.014)	0.268	(0.016)	-0.028	(0.021)	0.283	(0.015)	0.286	(0.015)	0.002	(0.021)
City	0.415	(0.015)	0.384	(0.018)	-0.031	(0.023)	0.424	(0.017)	0.383	(0.016)	-0.042	(0.023)
Region	39.30	(0.732)	40.48	(0.864)	1.176	(1.141)	40.31	(0.801)	39.28	(0.780)	-1.036	(0.559)

Mean comparison—prior and after reform, control and treatment group (*cont.*)

	Men											
	Prior to reform		**Post-reform**				**Below retirement age**		**Above retirement age**			
	Mean	**s.e.**	**Mean**	**s.e.**	**Difference**	**s.e.**	**Mean**	**s.e.**	**Mean**	**s.e.**	**Difference**	**s.e.**
Retired	0.542	(0.018)	0.497	(0.027)	-0.045	(0.033)	0.200	(0.019)	0.735	(0.017)	0.535	(0.026)
Age	60.49	(0.055)	60.01	(0.085)	-0.483	(0.100)	58.49	(0.024)	61.51	(0.019)	3.020	(0.031)
Married	0.906	(0.011)	0.924	(0.014)	0.017	(0.019)	0.913	(0.014)	0.911	(0.011)	-0.002	(0.018)
Widowed	0.048	(0.008)	0.035	(0.010)	-0.012	(0.013)	0.033	(0.009)	0.051	(0.008)	0.018	(0.013)
Years worked	36.77	(0.202)	35.46	(0.321)	-1.304	(0.370)	34.40	(0.281)	37.61	(0.204)	3.207	(0.340)
Years of schooling	11.11	(0.122)	11.79	(0.150)	0.680	(0.208)	11.94	(0.146)	10.92	(0.125)	-1.020	(0.196)
At least 12 yrs of schooling	0.390	(0.018)	0.488	(0.027)	0.099	(0.032)	0.504	(0.024)	0.368	(0.019)	-0.136	(0.030)
At least 14 yrs of schooling	0.221	(0.015)	0.247	(0.023)	0.026	(0.027)	0.264	(0.021)	0.207	(0.016)	-0.057	(0.026)
Household size	2.707	(0.044)	2.621	(0.062)	-0.086	(0.078)	2.732	(0.058)	2.647	(0.046)	-0.084	(0.074)
Children up to 17 in household	0.202	(0.015)	0.165	(0.020)	-0.037	(0.026)	0.198	(0.019)	0.186	(0.015)	-0.012	(0.024)
Person with invalidity status in household	0.045	(0.008)	0.041	(0.011)	-0.004	(0.013)	0.054	(0.011)	0.037	(0.007)	-0.017	(0.013)
Total income of other household members	668.56	(59.17)	1150.49	(159.03)	481.93	(138.37)	846.07	(109.70)	800.14	(78.86)	-45.93	(132.07)
Body Mass Index	26.16	(0.121)	26.47	(0.180)	0.315	(0.217)	26.14	(0.158)	26.33	(0.130)	0.192	(0.206)
Reduced physical activity	0.378	(0.021)	0.400	(0.032)	0.022	(0.038)	0.363	(0.029)	0.398	(0.022)	0.035	(0.036)
Chronic disease	0.069	(0.009)	0.074	(0.014)	0.005	(0.017)	0.049	(0.011)	0.083	(0.011)	0.034	(0.016)
Medical treatment	0.116	(0.012)	0.103	(0.017)	-0.013	(0.021)	0.097	(0.014)	0.122	(0.013)	0.025	(0.020)
Regular physical activity (sport)	0.153	(0.013)	0.188	(0.021)	0.035	(0.024)	0.184	(0.019)	0.152	(0.014)	-0.032	(0.023)
Village	0.383	(0.018)	0.388	(0.026)	0.005	(0.032)	0.374	(0.024)	0.391	(0.019)	0.017	(0.030)
Town	0.279	(0.016)	0.285	(0.025)	0.007	(0.029)	0.266	(0.021)	0.290	(0.018)	0.024	(0.028)
City	0.338	(0.017)	0.326	(0.025)	-0.012	(0.031)	0.360	(0.023)	0.318	(0.018)	-0.042	(0.029)
Region	40.17	(0.859)	39.63	(1.316)	-0.537	(1.556)	40.36	(1.152)	39.77	(0.921)	-0.592	(1.477)

Note: Standard errors in parentheses. Source: UHBS; author’s calculations.

Table 5

Differential treatment across subgroups; dependent variable: retired; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Men	**Women**	**Not chronic**	**Chronic**	**Impact of Min Service Years**	**Low impact region**	**High impact region**	**Urban**	**Rural**
Treatment effect	0.176***	0.133***	0.144***	0.078		0.120**	0.182***	0.153***	0.105**
	(0.059)	(0.041)	(0.034)	(0.174)		(0.047)	(0.046)	(0.042)	(0.050)
Retirement age	0.474***	0.440***	0.450***	0.490***		0.412***	0.495***	0.376***	0.621***
	(0.035)	(0.028)	(0.022)	(0.085)		(0.028)	(0.032)	(0.026)	(0.034)
Post-reform	-0.049	-0.034	-0.045**	0.141	0.407	0.000	-0.098***	-0.039	-0.045
	(0.042)	(0.021)	(0.020)	(0.149)	(0.075)	(0.026)	(0.031)	(0.024)	(0.038)
Min service years (MSY)					0.429*** (0.047)				
MSY*post-reform					-0.183**				
					(0.075)				
Chronic					0.127				
					(0.139)				
MSY*Chronic					-0.097				
					(0.142)				
MSY*Post-reform*Chronic					0.189*** (0.073)				
Constant	0.215***	0.111***	0.223***	0.322***	1.273***	0.212***	0.249***	0.233***	0.207***
	(0.027)	(0.015)	(0.019)	(0.081)	(0.115)	(0.026)	(0.028)	(0.024)	(0.030)
Observations	1097	1845	2781	161	4416	1501	1441	1943	999
R-squared	0.272	0.271	0.282	0.389	0.290	0.266	0.322	0.236	0.433
F test	16.4		3.0			18.5		37.8	

Note:Linear probability models with dependent variable: retired. F test for hypothesis that coefficients are significantly different for two comparison groups in (1), (2) and (4). Regression (3) is a pooled regression containing interactions between Minimum Service Years (20 for women, 25 for men), post-reform period and chronic. Sample is extended to five pre-retirement years during which the majority of early retirement takes place. Regression controls for full set of controls including year of birth dummies (see Table 4). Critical F-value for 2942 observations is 2.37. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations

Table 6
Robustness checks; dependent variable: retired; UHBS data

	(1) **Men**	(2) **Women**	(3) **Men**	(4) **Men**	(5) **Women**	(6) **Women**
	—excluding mining area		**—controlling for shadow wage**			
Treatment effect	0.158***	0.127***	0.152***	0.143***	0.123***	0.110***
	(0.061)	(0.042)	(0.058)	(0.055)	(0.040)	(0.038)
Retirement age	0.473***	0.444***	0.457***	0.216**	0.428***	0.297***
	(0.036)	(0.029)	(0.035)	(0.088)	(0.027)	(0.062)
Post-reform	-0.040	-0.038*	0.067	0.104	0.063**	-0.037
	(0.043)	(0.022)	(0.047)	(0.074)	(0.025)	(0.056)
Shadow wage (yearly earnings)			-0.069***	-0.081*	-0.067***	0.003
			(0.013)	(0.044)	(0.009)	(0.037)
Constant	0.210***	0.117***	0.339***	-59.096*	0.198***	25.547
	(0.027)	(0.015)	(0.036)	(31.621)	(0.019)	(18.562)
Full controls	—	—	—	X	—	X
Observations	1050	1748	1097	1097	1845	1845
R-squared	0.266	0.270	0.297	0.386	0.296	0.385

Note:Linear probability models with dependent variable: retired. Columns (1) and (2): Mining areas are regions in which more than 20 percent of regional employment is concentrated in the mining sector (3 out of 78). Columns (3)-(6): Shadow wage caluclated as potential yearly earnings in gender-age-education-region cell, correcting for labor force particpation. These cells contain predictions from a Heckit models which accounts for selection into the working state by exploiting pension age as an exclusion restriction. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations

Table 7
Impact of pension increase on household composition; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled		**Men**		**Women**	
	Dependent variable					
	Household size	**Number of working age household members**	**Household size**	**Number of working age household members**	**Household size**	**Number of working age household members**
Treatment effect	-0.062	0.050	0.070	0.085	-0.149	0.008
	(0.077)	(0.062)	(0.105)	(0.086)	(0.096)	(0.078)
Retirement age	0.039	-0.968***	0.177	-0.983***	0.135	-1.048***
	(0.051)	(0.043)	(0.151)	(0.130)	(0.151)	(0.120)
Post-reform	0.070	0.016	-0.088	-0.162**	0.147**	0.098*
	(0.057)	(0.046)	(0.080)	(0.069)	(0.072)	(0.056)
Constant	5.405	7.343	46.591	-42.617	38.226	9.454
	(7.620)	(6.142)	(57.212)	(51.532)	(46.920)	(37.915)
Observations	2942	2942	1097	1097	1845	1845
R-squared	0.587	0.558	0.626	0.573	0.572	0.548

Note: Linear regressions controlling for region and place of settlement, age, marital status, education, work experience, chronic disease, presence of children up to 17 in household, presence of person with invalidity status in household, regional industry structure (share of employment in mining, agriculture, state enterprises as well as unemployment rate). Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 8
Labor supply effect of pension increase; dependent variable: retired; ULMS data

	(1) **Women, 3 years**	(2) **Men, 3 years**	(3) **Pooled, no household re-formation**	(4) **Pooled, with household re-formation**
Treatment effect	0.146**	0.223**	0.150***	0.139***
	(0.0573)	(0.104)	(0.045)	(0.041)
Retirement age	0.337***	0.355***	0.344***	0.332***
	(0.041)	(0.049)	(0.029)	(0.024)
Post-reform	0.059	0.023	0.045	0.041
	(0.0456)	(0.060)	(0.036)	(0.033)
Constant	0.137	0.199	0.156	0.150
	(0.433)	(0.477)	(0.323)	(0.281)
Observations	713	365	1078	1339
R-squared	0.171	0.159	0.156	0.168

Note: Regressions control for age dummies, marital status, education, chronic diseases, household size, presence of children in household, income generated by other household members, region of settlement. (3) and (4) include a gender dummy. Age brackets +/- 3 age cohorts around retirement age with year of retirement age excluded. Retirement aged reflects retirement eligibility. Column (1) to (3) exclude households which changed composition between 2004 and 2007. Robust standard errors clustered by household size in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003, 2004, 2007; author’s calculations.

Table 9
Retirement and eligibility of couples; UHBS data

Age of Wive		Age of Husband 50-54	55-59	60-64	65-69	70-74
50-54	**2003**	7.4%	16.7%	59.6%	**a**	**a**
	2005	9.6%	13.0%	50.0%	73.7%	**a**
	sig.	*	**	*		
55-59	**2003**	41.0%	46.6%	76.5%	88.5%	**a**
	2005	42.0%	54.8%	81.9%	86.7%	**a**
	sig.		**	**		
60-64	**2003**	**a**	82.6%	88.9%	93.8%	100%
	2005	**a**	81.3%	89.2%	92.9%	100%
	sig.					
65-69	**2003**	**a**	**a**	92.1%	95.6%	97.6%
	2005	**a**	**a**	96.2%	96.6%	95.3%
	sig.			*		*
70-74	**2003**	**a**	**a**	**a**	97.1%	99.1%
	2005	**a**	**a**	**a**	100%	100%
	sig.				*	

Note: a. Less than 40 observations in cell. Cells report share of couples with at least one partner retired. Framed numbers contain between 30 and 40 observations only. Shaded area marks retirement eligibility of at least one partner. *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS 2003 and 2005; author's caluclations.

Table 10
Share of jointly retired couples; UHBS data

Age of Wive		Age of Husband 50-54	55-59	60-64	65-69	70-74
50-54	**2003**	14.3%	17.6%	9.7%	**a**	**a**
	2005	10.3%	12.9%	5.9%	**a**	**a**
	sig.					
55-59	**2003**	0.0%	16.2%	53.8%	72.2%	**a**
	2005	4.8%	11.4%	65.6%	75.0%	**a**
	sig.		*	***		
60-64	**2003**	**a**	21.1%	75.0%	76.4%	83.0%
	2005	**a**	11.5%	77.4%	79.8%	81.5%
	sig.					
65-69	**2003**	**a**	**a**	75.9%	77.7%	88.4%
	2005	**a**	**a**	76.5%	83.2%	88.8%
	sig.				**	
70-74	**2003**	**a**	**a**	**a**	93.9%	91.8%
	2005	**a**	**a**	**a**	88.6%	93.8%
	sig.					

Note: a. Less than 40 observations in cell. Cells report share of jointly retired couples in all couples with at least one partner retired. Framed numbers contain between 30 and 40 observations only. Shaded area marks age of joint normal retirement age. *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS 2003 and 2005; author's caluclations.

Table 11
Difference-in-Differences of retirement—choice of comparison bandwidth; dependent variable: retired; UHBS data

	(1) 1 year	(2) 2 years	(3) 3 years	(4) 4 years	(5) 5 years
			Men		
Treatment effect	0.223***	0.176***	0.146***	0.118***	0.105***
	(0.086)	(0.059)	(0.045)	(0.037)	(0.031)
Constant	0.297***	0.215***	0.199***	0.184***	0.166***
	(0.044)	(0.027)	(0.021)	(0.018)	(0.014)
Observations	538	1097	1729	2472	3226
R-squared	0.194	0.272	0.311	0.340	0.381
			Women		
Treatment effect	0.101*	0.133***	0.091***	0.077***	0.057**
	(0.057)	(0.041)	(0.033)	(0.028)	(0.025)
Constant	0.124***	0.111***	0.099***	0.084***	0.073***
	(0.021)	(0.015)	(0.011)	(0.009)	(0.007)
Observations	996	1845	2675	3555	4398
R-squared	0.216	0.271	0.318	0.372	0.414

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 12
Retirement rates across survey years

	Men		**Women**	
Age groups	58/59	61/62	53/54	56/57
2002	0.187	0.692	0.129	0.536
2003	0.213	0.687	0.094	0.564
	(0.63)	-(0.12)	-(1.18)	(0.59)
2004	0.203	0.715	0.100	0.633
	(0.40)	(0.46)	-(0.97)	(2.13)
2005	0.163	0.816	0.090	0.652
	-(0.62)	(2.68)	-(1.41)	(2.72)
2006	0.198	0.804	0.110	0.659
	(0.30)	(2.39)	-(0.62)	(2.90)

Note: Report values are retirement rates. T-statistics in parentheses for a test of the hypothesis that year coefficients are statistically significant different from the base category (2002). Source: UHBS; author's calculations.

Table 13
Instrumental variable estimation of the effect of pension receipt on retirement; dependent variable: not working; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full sample			**Men**			**Women**		
	OLS	**IV**	**First stage**	**OLS**	**IV**	**First stage**	**OLS**	**IV**	**First stage**
Pension Receiver	0.359***	0.427***		0.412***	0.644***		0.363***	0.439**	
	(0.020)	(0.041)		(0.031)	(0.073)		(0.038)	(0.176)	
Pension eligible*post-reform			0.679***			0.665***			0.223***
			(0.024)			(0.046)			(0.026)
Constant	-0.749***	-0.488*	-2.976***	1.319***	1.297***	0.253	-1.039	0.000	-12.30***
	(0.225)	(0.266)	(0.179)	(0.200)	(0.200)	(0.189)	(0.661)	(2.443)	(0.307)
Observations	2942	2942	2942	1097	1097	1097	1845	1845	1845
F-stat			77.9			209.9			71.5
R-squared	0.325	0.321		0.314	0.274		0.338	0.336	
Partial R-squared			0.212			0.166			0.038

Note: Dependent variable: retired. All regressions control for full set of controls (see Table 4). Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 14
Difference-in-Regression-Discontinuity estimation; dependent variable: retired; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)
			Men			
Treatment effect	0.188***	0.187***	0.176***	0.176***	0.174***	0.175***
	(0.059)	(0.058)	(0.057)	(0.057)	(0.057)	(0.057)
Norm. age	0.058***	0.060***	0.068***	0.067***	0.066***	0.066***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Norm. age squ.	0.003*	0.003**	0.003**	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Retirement age	0.315***	0.309***	0.315***	0.315***	0.319***	0.317***
	(0.065)	(0.063)	(0.062)	(0.062)	(0.062)	(0.062)
Norm. age*retirement age	-0.013	-0.018	-0.026	-0.026	-0.025	-0.024
	(0.029)	(0.029)	(0.028)	(0.028)	(0.028)	(0.028)
Norm. age squ.*retirement age	-0.004***	-0.004***	-0.004***	-0.004***	-0.004***	-0.004***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Post-reform	-0.054	-0.063	-0.058	-0.058	-0.056	-0.056
	(0.065)	(0.064)	(0.064)	(0.063)	(0.063)	(0.063)
Norm. age*post-reform	-0.006	-0.010	-0.007	-0.007	-0.007	-0.006
	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Norm. age squ.*post-reform	0.000	-0.000	0.000	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.323***	0.248***	0.786***	0.782***	0.803***	0.639***
	(0.057)	(0.059)	(0.075)	(0.074)	(0.075)	(0.095)
Observations	4690	4690	4690	4690	4690	4690
R-squared	0.571	0.585	0.601	0.602	0.603	0.604
			Women			
Treatment effect	0.103**	0.097**	0.088**	0.088**	0.086**	0.086**
	(0.044)	(0.043)	(0.041)	(0.041)	(0.041)	(0.041)
Norm. age	0.029***	0.026***	0.034***	0.034***	0.033***	0.033***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Norm. age squ.	0.002***	0.001***	0.001**	0.001**	0.001**	0.001**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Retirement age	0.336***	0.344***	0.348***	0.348***	0.350***	0.351***
	(0.047)	(0.046)	(0.045)	(0.045)	(0.045)	(0.045)
Norm. age*retirement age	0.051***	0.050***	0.048***	0.048***	0.048***	0.048***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Norm. age squ.*retirement age	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post reform	-0.024	-0.019	-0.018	-0.017	-0.016	-0.015
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Norm. age*post-reform	-0.007**	-0.006**	-0.004	-0.004	-0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Norm. age squ.*post-reform	-0.000	-0.000*	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.138***	0.085***	0.603***	0.602***	0.617***	0.502***
	(0.022)	(0.031)	(0.049)	(0.049)	(0.051)	(0.069)
Observations	6762	6762	6762	6762	6762	6762
R-squared	0.618	0.634	0.653	0.653	0.653	0.653
Region & Place FE	—	X	X	X	X	X
Individual controls	—	—	X	X	X	X
Health controls	—	—	—	X	X	X
Household controls	—	—	—	—	X	X
Industry structure	—	—	—	—	—	X

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations

Table 15
Means of labor supply—by age group and reform exposure

Men	**2003-2004**	**2007**		**Women**	**2003-2004**	**2007**		**Least educated**	**2003-2004**	**2007**	
	Pre-reform	**Post-reform**	**Difference**		**Pre-reform**	**Post-reform**	**Difference**		**Pre-reform**	**Post-reform**	**Difference**
Panel A: Dependent variable: Yearly working hours											
Age 58-59	2086.042	2074.018	-12.024	Age 53-54	1626.93	1649.87	22.94	Age 53-54	1360.62	1333.55	-27.07
	(95.825)	(105.697)	(45.359)		(251.89)	(257.89)	(55.53)		(415.32)	(435.30)	(112.82)
Age 61-62	1879.790	1982.177	102.387	Age 56-57	1834.58	1577.06	-257.52	Age 56-57	1414.11	926.99	-487.12
	(42.254)	(66.339)	(64.604)		(249.30)	(245.93)	(66.23)		(337.84)	(371.09)	(163.56)
Difference	-206.252	-91.841	114.411	Difference	207.65	-72.81	-280.46	Difference	53.50	-406.55	-460.05
N=902	(90.912)	(100.655)	(80.650)	N=976	(74.08)	(86.59)	(86.01)	N=211	(181.84)	(244.94)	(200.69)
Panel B: Dependent variable: Yearly working weeks											
Age 58-59	48.856	49.541	0.685	Age 53-54	45.636	46.695	1.059	Age 53-54	38.888	40.525	1.636
	(1.109)	(1.208)	(0.484)		(1.362)	(1.445)	(55.528)		(4.854)	(4.755)	(1.332)
Age 61-62	48.180	49.374	1.194	Age 56-57	47.855	45.259	-2.595	Age 56-57	42.328	36.552	-5.776
	(0.539)	(1.127)	(1.207)		(1.014)	(1.289)	(0.791)		(3.769)	(4.289)	(2.107)
Difference	-0.677	-0.167	0.510	Difference	2.218	-1.436	-3.655	Difference	3.440	-3.973	-7.413
N=902	(1.163)	(0.713)	(1.299)	N=976	(0.892)	(0.915)	(0.917)	N=211	(2.509)	(2.458)	(2.564)
Panel C: Dependent variable: Weekly working hours											
Age 58-59	42.126	40.834	-1.292	Age 53-54	34.232	33.356	-0.876	Age 53-54	32.918	29.138	-3.780
	(1.968)	(2.114)	(0.789)		(4.737)	(4.858)	(1.099)		(7.728)	(8.235)	(2.357)
Age 61-62	39.258	39.262	0.004	Age 56-57	36.966	33.162	-3.804	Age 56-57	31.418	23.327	-8.091
	(1.077)	(1.408)	(1.212)		(4.716)	(4.655)	(1.275)		(6.246)	(6.634)	(3.114)
Difference	-2.868	-1.572	1.295	Difference	2.734	-0.195	-2.929	Difference	-1.500	-5.811	-4.311
N=902	(1.763)	(1.891)	(1.443)	N=976	(1.315)	(1.563)	(1.671)	N=211	(3.388)	(4.902)	(3.870)

Source: ULMS; author’s calculations.

Table 16
Difference-in-Differences of yearly working hours; ULMS data

	(1)	(2)	(3)	(4)
	Dependent variable: Yearly working hours			
	Full sample	Men	Women	Educational category 1
No Controls				
Treatment effect	-94.952	114.412	-280.456***	-460.051**
	(59.628)	(80.650)	(86.011)	(200.687)
Constant	1,722.589***	2,086.042***	1,626.933***	1,360.617***
	(122.949)	(95.825)	(251.895)	(415.323)
Goodness of fit (ρ^2)	0.178	0.169	0.109	0.041
Observations	1877	902	976	211
Number of truncated observations	2794	999	1795	872
Full controls				
Treatment effect	-119.986**	50.900	-281.119***	-449.022**
	(60.884)	(81.482)	(84.860)	(226.291)
Constant	1,924.744*	2,799.480**	917.383	1,868.485
	(1,084.404)	(1,374.398)	(798.138)	(1,802.273)
Goodness of fit (ρ^2)	0.049	0.058	0. 045	0.061
Observations	1740	833	906	192
Number of truncated observations	2623	941	1682	815

Note: Table reports estimates from a truncated linear regression, truncation at zero. Regressions with no controls include a gender dummy and year of birth fixed effects. Full controls include region and settlement type fixed effects, age, years of schooling, marital status (married, widowed, single or seperated), a dummy for one out of seven chronic diseases, children up to age 17 present in household, household size, total income of other household members. Robust standard errors in parentheses, clustered by id; *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS; author's calculation.

Table 17
Labor supply responses at intensive margin, women sample; ULMS data

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Yearly working hours					
Treatment effect	-297.641***	-300.563***	-288.328***	-277.110***	-281.119***
	(87.881)	(85.657)	(85.154)	(84.665)	(84.860)
Constant	1,999.642***	1,929.807***	873.583	894.096	917.383
	(195.489)	(209.033)	(774.364)	(782.222)	(798.138)
Observations	906	906	906	906	906
Goodness of fit (ρ^2)	0.014	0.024	0.039	0.044	0.045
Dependent variable: Yearly working weeks					
Treatment effect	-3.577***	-3.566***	-3.419***	-3.400***	-3.264***
	(0.986)	(0.960)	(0.938)	(0.930)	(0.935)
Constant	45.740***	45.910***	25.524***	27.407***	28.150***
	(1.174)	(1.535)	(6.911)	(6.981)	(6.749)
Observations	906	906	906	906	906
Goodness of fit (ρ^2)	0.007	0.010	0.016	0.015	0.018
Dependent variable: Weekly working hours					
Treatment effect	-3.013*	-2.961*	-2.870*	-2.678*	-2.722*
	(1.664)	(1.641)	(1.615)	(1.614)	(1.602)
Constant	36.989***	44.196***	41.375*	37.876*	41.923**
	(2.970)	(4.330)	(21.344)	(21.101)	(20.752)
Observations	906	906	906	906	906
Goodness of fit (ρ^2)	0.010	0.027	0.035	0.029	0.043
Region & Place FE	—	X	X	X	X
Individual controls	—	—	X	X	X
Health controls	—	—	—	X	X
Household controls	—	—	—	—	X

Note: Table reports estimates from a truncated linear regression, truncation at zero. Regressions with no controls include year of birth fixed effects. Full controls include region and settlement type fixed effects, age, years of schooling, marital status (married, widowed, single or seperated), a dummy for one out of seven chronic diseases, children up to age 17 present in household, household size, total income of other household members. Robust standard errors in parentheses, clustered by id; *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS; author's calculation.

Table 18
Labor supply responses at intensive margin, least educated sample; ULMS data

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Yearly working hours					
Treatment effect	-363.348*	-381.060*	-375.622*	-361.343*	-449.022**
	(204.588)	(198.540)	(196.327)	(196.616)	(226.291)
Constant	1,257.841***	1,010.884**	2,317.049	2,364.564	1,868.485
	(463.954)	(464.929)	(1,652.760)	(1,695.076)	(1,802.273)
Observations	192	192	192	192	192
Goodness of fit (ρ^2)	0.056	0.036	0.065	0.068	0.061
Dependent variable: Yearly working weeks					
Treatment effect	-7.324**	-8.356**	-8.313**	-8.339**	-6.934**
	(2.934)	(3.397)	(3.492)	(3.664)	(2.851)
Constant	42.503***	49.107***	59.892***	59.858***	58.697***
	(6.735)	(1.814)	(22.748)	(22.608)	(22.005)
Observations	192	192	192	192	192
Goodness of fit (ρ^2)	0.006	0.004	0.018	0.017	0.013
Dependent variable: Weekly working hours					
Treatment effect	-2.044	-2.499	-2.258	-2.403	-3.454
	(3.257)	(3.308)	(3.513)	(3.478)	(4.240)
Constant	40.987***	60.344***	-1.230	-1.421	5.665
	(7.354)	(12.363)	(29.430)	(29.065)	(29.426)
Observations	192	192	192	192	192
Goodness of fit (ρ^2)	0.008	0.012	0.033	0.033	0.032
Region & Place FE	—	X	X	X	X
Individual controls	—	—	X	X	X
Health controls	—	—	—	X	X
Household controls	—	—	—	—	X

Note: Table reports estimates from a truncated linear regression, truncation at zero. Regressions with no controls include a gender dummy and year of birth fixed effects. Full controls include region and settlement type fixed effects, age, years of schooling, marital status (married, widowed, single or seperated), a dummy for one out of seven chronic diseases, children up to age 17 present in household, household size, total income of other household members. Educational category 1 means primary and unfinished education. Robust standard errors in parentheses, clustered by id; *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS; author's calculation.

Table 19
Robustness checks for labor supply responses at intensive margin; dependent variable: yearly working hours; ULMS data

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		**Sub sample of (1)**			
	Baseline	Random effects	Controlling for occupation 1986	Chronic=0	Chronic=1	Only households without change in composition
Women						
Treatment effect	-265.72***	-228.82***	-260.28***	36.36	-354.37***	-244.11***
	(84.09)	(77.01)	(89.84)	(179.83)	(88.97)	(91.26)
Constant	1,267.32		1,536.88	2,315.83*	225.90	954.49
	(942.77)		(1,055.27)	(1,395.24)	(1,160.70)	(1,064.69)
Observations	906	906	832	249	657	713
	0.003		0.000	0.000	0.018	0.013
R-squared		0.132				
Hausman test						
Prob>chi2		0.18				
Least educated						
Treatment effect	-449.02**	-459.74*	-375.52	-831.15*	-201.13	-457.54**
	(226.29)	(256.26)	(259.69)	(424.97)	(225.67)	(221.71)
Constant	1,868.49		1,523.65	-7,446.11***	1,401.72	3,340.97*
	(1,802.27)		(1,493.43)	(2,782.26)	(2,422.28)	(1,744.56)
Observations	192	192	173	60	132	156
	0.061		0.046	0.021	0.054	0.076
R-squared		0.282				
Hausman test						
Prob>chi2		0.99				

Note: All regressions include full set of controls (see Table 13). Regressions (1) and (3)-(6) are truncated linear regressions. Standard error clustered by id. Regression (2) is a random effects panel regression. The Hausman statistics test the null hypothesis that there are no systematic differences in coefficients from random effects vs. fixed effects model (the latter not shown). *** p<0.01, ** p<0.05, * p<0.1. Source ULMS; author's calculations.

Table 20

Difference-in-Differences of working weeks and weekly working hours; ULMS data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: Yearly working weeks				**Dependent variable: Weekly working hours**			
	Full sample	Men	Women	Educational category 1	Full sample	Men	Women	Educational category 1
No Controls								
Treatment effect	-1.619**	0.510	-3.655***	-7.413***	-0.853	1.295	-2.929*	-4.311
	(0.703)	(1.300)	(0.917)	(2.564)	(1.117)	(1.443)	(1.671)	(3.870)
Constant	47.671***	48.856***	45.636***	38.888***	41.963***	42.126***	34.232***	32.918***
	(1.209)	(1.190)	(1.023)	(4.854)	(0.416)	(1.968)	(4.737)	(7.728)
Goodness of fit (ρ^2)	0.021	0.009	0.022	0.011	0.019	0.017	0.017	0.014
Observations	1877	902	976	211	1877	902	976	211
Truncated observations	2794	999	1795	872	2794	999	1795	872
Full controls								
Treatment effect	-1.655**	0.081	-3.264***	-6.934**	-1.014	1.175	-2.722*	-3.451
	(0.707)	(1.330)	(0.935)	(2.851)	(1.068)	(1.450)	(1.602)	(4.240)
Constant	46.742***	77.838***	28.150***	58.697***	46.582*	63.268**	41.923*	5.665
	(16.407)	(23.890)	(6.749)	(22.005)	(26.562)	(29.965)	(20.752)	(29.426)
Goodness of fit (ρ^2)	0.027	0.009	0.018	0.013	0.063	0.032	0.043	0.032
Observations	1740	833	906	192	1740	833	906	192
Truncated observations	2623	941	1682	815	2623	941	1682	815

Note: Table reports estimates from a truncated linear regression, truncation at zero. Regressions with no controls include a gender dummy and year of birth fixed effects. Full controls include region and settlement type fixed effects, age, years of schooling, marital status (married, widowed, single or seperated), a dummy for one out of seven chronic diseases, children up to age 17 present in household, household size, total income of other household members. Educational category 1 means primary and unfinished education. Robust standard errors in parentheses, clustered by id; *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS; author's calculation.

Table 21
Net present total compensation at retirement age and retirement incentives across educational categories

		2003	Cost of immediate retirement %	2005	Cost of immediate retirement %	Difference
Men (life expectancy at retirement 14 years)						
Lower education	Working 3 more years	6,286		10,547		
	Immediate retirement	4,312	31.4%	8,394	20.4%	-35.0%
Completed secondary education	Working 3 more years	6,410		11,398		
	Immediate retirement	4,319	32.6%	8,451	25.9%	-20.8%
Higher education	Working 3 more years	6,836		12,560		
	Immediate retirement	4,320	36.8%	8,871	29.4%	-20.2%
Women (life expectancy at retirement 25 years)						
Lower education	Working 3 more years	7,601		14,429		
	Immediate retirement	6,221	18.2%	12,730	11.8%	-35.2%
Completed secondary education	Working 3 more years	8,092		14,892		
	Immediate retirement	6,647	17.9%	12,753	14.4%	-19.6%
Higher education	Working 3 more years	8,649		15,911		
	Immediate retirement	6,647	23.1%	12,982	18.4%	-20.5%

Notes: Total compensation is calculated assuming a constant interest rate of 3%, constant across gender and educational level.
Life expectancy at retirement varies with gender but is assumed constant across educational levels. Potential earnings are computed as median value for married individuals residing in non-rural areas. Yearly retirement benefits are computed at the median of educational groups and are assumed constant over time. Some government sources mentioned that pensions were indexed to inflation plus a further amount of not less than 20 percent in the increase in the national average wage, however, as the implementation of indexation remained unclear at that time we assume constant values. In reality, the indexation includes 20 percent of real wage growth since March 2005. Values report discounted total compensation until death in 2002 USD PPP. Life expectancy at retirement age is taken from Gora (2008).

Table 22
Difference-in-Differences in educational CDF; dependent variable: retired; UHBS data

Years of schooling	DiD in CDF	Robust s.e.
6	0.403	(0.03)
7	0.363	(0.03)
8	0.361	(0.05)
9	0.170	(0.22)
10	0.232	(0.04)
11	0.215	(0.06)
12	0.084	(0.06)
13	0.104	(0.06)
14	0.138	(0.11)
15	-0.081	(0.07)
16	-0.101	(0.14)
17	-0.009	(0.15)

Note: Reported values are regression coefficients on interactions between years of schooling and the treatment indicator. Linear regressions are performed on pooled male and female sample in order to increase estimation precision. Small sample size for 6 and 9 years of schooling. Robust standard errors in parentheses for the hypotheses that DiD coefficients are signficantly different from the control group. Regressions control for age, year and gender dummies as well as for marital status. Source: UHBS; author's calculations.

Table 23
Robustness check: Difference-in-Differences in pension gain; dependent variable: retired; UHBS data

	(1) **Below median potential pension growth**	(2) **Above median potential pension growth**	(3) **Men—controlling for potential pension growth**	(4) **Women—controlling for potential pension growth**
Treatment effect	0.134***	0.169***	0.164***	0.130***
	(0.043)	(0.051)	(0.059)	(0.040)
Retirement age	0.403***	0.536***	0.471***	0.432***
	(0.027)	(0.035)	(0.035)	(0.028)
Post-reform	-0.034	-0.054	-0.041	-0.035
	(0.024)	(0.037)	(0.043)	(0.021)
Potential pension growth			0.240**	0.421***
			(0.101)	(0.055)
Average predicted pension growth			142%	165%
Constant	0.221***	0.233***	-0.115	-0.540***
	(0.024)	(0.030)	(0.146)	(0.084)
Observations	1886	1056	1097	1845
R-squared	0.245	0.358	0.276	0.293

Note:Linear probability models with dependent variable: retired. Potential pension growth is calculated as growth rate in predicted pension benefits between 2003 and 2005 for specific gender, education, regional and settlement type groups. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations

Table 24

Effect of pension increase on absolute and relative deprivation; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probability of exceeding absolute poverty line		**Relative position to mean**		**Probability of exceeding absolute poverty line**		**Relative position to mean**	
	Narrow group	Broad group	Narrow group	Broad group	Narrow group	Broad group	Narrow group	Broad group
Treatment effect	0.163***	0.227***	0.185**	0.135**	0.190***	0.229***	0.242***	0.132***
	(0.046)	(0.033)	(0.083)	(0.053)	(0.041)	(0.029)	(0.074)	(0.047)
Retirement age	0.041	0.005	0.074	-0.005	0.019	-0.014	0.055	0.002
	(0.029)	(0.019)	(0.046)	(0.034)	(0.026)	(0.018)	(0.040)	(0.031)
Post-reform	0.362***	0.326***	-0.049	-0.094**	0.333***	0.310***	-0.126**	-0.136***
	(0.037)	(0.029)	(0.058)	(0.046)	(0.033)	(0.026)	(0.049)	(0.040)
Constant	0.262***	0.264***	1.014***	1.060***	-0.423***	-0.330***	0.015	0.194**
	(0.022)	(0.016)	(0.037)	(0.030)	(0.098)	(0.059)	(0.163)	(0.094)
Full controls	—	—	—	—	X	X	X	X
Observations	2016	5026	2016	5026	2016	5026	2016	5026
R-squared	0.200	0.228	0.012	0.002	0.355	0.355	0.262	0.220

Note: Regressions (1), (2), (5) and (6) are linear probability models. Regressions for full sample of men and women. Narrow group comprises one year prior and one year post retirement age. Broad group comprises two years prior and four years post retirement age. Probability of exceeding absolute poverty line compares total individual disposable income to the 2.15 USD absolute poverty line (PPP adjusted). Relative position calculated with respect to the gender specific yearly mean of total individual disposable income. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 25
Robustness checks 1 & 2; dependent variable: retired; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)
Robustness check 1: Probit specification, marginal effects reported						
Men						
Treatment effect	0.226***	0.213***	0.209**	0.206**	0.223***	0.225***
	(0.076)	(0.079)	(0.083)	(0.083)	(0.081)	(0.081)
Observations	1097	1097	1097	1097	1097	1097
Pseudo R-squared	0.209	0.263	0.310	0.316	0.325	0.328
Women						
Treatment effect	0.170***	0.173***	0.147**	0.147**	0.151**	0.152**
	(0.061)	(0.063)	(0.064)	(0.064)	(0.064)	(0.065)
Observations	1845	1845	1845	1845	1845	1845
Pseudo R-squared	0.226	0.285	0.347	0.347	0.348	0.352
Robustness check 2: Omission of those below minimum working year threshold						
Men						
Treatment effect	0.180***	0.160***	0.162***	0.157***	0.163***	0.163***
	(0.061)	(0.060)	(0.058)	(0.058)	(0.056)	(0.056)
Constant	0.226***	0.174**	-56.762*	-54.972*	-56.023*	-56.862*
	(0.028)	(0.078)	(32.678)	(32.540)	(32.414)	(32.392)
Observations	1063	1063	1063	1063	1063	1063
R-squared	0.260	0.317	0.372	0.376	0.386	0.388
Women						
Treatment effect	0.137***	0.125***	0.098**	0.097**	0.100***	0.103***
	(0.041)	(0.040)	(0.038)	(0.038)	(0.038)	(0.039)
Constant	0.115***	0.057	25.209	25.069	25.774	23.858
	(0.015)	(0.061)	(18.700)	(18.724)	(18.707)	(18.620)
Observations	1806	1806	1806	1806	1806	1806
R-squared	0.266	0.321	0.388	0.388	0.389	0.392
Region & Place FE	—	X	X	X	X	X
Individual controls	—	—	X	X	X	X
Health controls	—	—	—	X	X	X
Household controls	—	—	—	—	X	X
Industry structure	—	—	—	—	—	X

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 26
Robustness checks 3 & 4; dependent variable: retired; UHBS data

	(1)	(2)	(3)	(4)	(5)	(6)
Robustness check 3: Comparison 2002/03 vs. 2004/05						
Men						
Treatment effect	0.114**	0.101**	0.088*	0.085*	0.089*	0.090*
	(0.049)	(0.048)	(0.047)	(0.047)	(0.047)	(0.047)
Constant	0.215***	0.142**	-51.110*	-49.833*	-50.366*	-51.078*
	(0.027)	(0.067)	(28.093)	(28.090)	(27.952)	(27.901)
Observations	1436	1436	1436	1436	1436	1436
R-squared	0.273	0.311	0.354	0.357	0.363	0.364
Women						
Treatment effect	0.113***	0.102***	0.088***	0.087***	0.089***	0.090***
	(0.036)	(0.035)	(0.033)	(0.033)	(0.033)	(0.033)
Constant	0.111***	0.044	24.929	24.946	25.207	24.155
	(0.015)	(0.048)	(16.228)	(16.225)	(16.222)	(16.151)
Observations	2465	2465	2465	2465	2465	2465
R-squared	0.280	0.333	0.380	0.380	0.380	0.383
Robustness check 4: Comparison 2002 vs. 2005						
Men						
Treatment effect	0.127**	0.106*	0.120*	0.120*	0.115*	0.115*
	(0.061)	(0.062)	(0.062)	(0.062)	(0.061)	(0.062)
Constant	0.185***	0.099	-56.734	-53.933	-52.220	-52.989
	(0.034)	(0.087)	(36.587)	(36.596)	(36.600)	(36.687)
Observations	717	717	717	717	717	717
R-squared	0.342	0.387	0.412	0.415	0.420	0.422
Women						
Treatment effect	0.165***	0.172***	0.149***	0.149***	0.154***	0.152***
	(0.050)	(0.049)	(0.047)	(0.047)	(0.047)	(0.047)
Constant	0.129***	0.137*	41.624*	41.573*	42.279*	40.355*
	(0.022)	(0.081)	(22.349)	(22.374)	(22.446)	(22.499)
Observations	1257	1257	1257	1257	1257	1257
R-squared	0.281	0.343	0.399	0.399	0.401	0.403
Region & Place FE	—	X	X	X	X	X
Individual controls	—	—	X	X	X	X
Health controls	—	—	—	X	X	X
Household controls	—	—	—	—	X	X
Industry structure	—	—	—	—	—	X

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS; author's calculations.

Table 27
Data Overview ULMS

	Pre-reform period			Post-reform period		
	mean	min	max	mean	min	max
Yearly working hours	1959.1	0	4992	1919.8	0	4680
Actual working hours reference week	38.8	0	98	39.3	0	90
Normal weekly working hours	41.2	3	98	40.2	0	90
Yearly working weeks	47.47	0	52	47.47	4	52
Share working less than full-time	0.061	0	1	0.073	0	1

	Pre-reform period			Post-reform period		
	mean	min	max	mean	min	max
Male	0.383	0	1	0.376	0	1
Married	0.786	0	1	0.743	0	1
Age	53.8	43	65	57.5	47	68
Chronic disease	0.676	0	1	0.680	0	1
Years of schooling	11.6	4	15	11.6	4	15
Household size	3.1	1	13	3.0	1	9
Presence of children (0-17 years)	0.307	0	1	0.265	0	1
Income from other household members	492.7	0	8650	1088.7	0	8376.1
Kiev	0.038	0	1	0.041	0	1
East	0.268	0	1	0.260	0	1
West	0.197	0	1	0.204	0	1
Center	0.272	0	1	0.277	0	1
South	0.191	0	1	0.218	0	1
Rural	0.362	0	1	0.369	0	1

Note: Number of observations in pre-reform period is 1,252 and in post-reform period is 626. Source: ULMS; author's calculations

Appendix: Changes in cohort densities and educational distribution

The RD estimator will only be unbiased under the assumption that we compare very similar people. However, as the UHBS data are formed of cross-sections, the cohorts change as time passes. The cohort aged 60 in 2003 will be 62 in 2005 and so the counterfactual is not straightforward to determine. As shown in Table 2, the comparison cohorts are very similar with respect to observable characteristics, however, two problems arise: First, the educational composition of the cohorts under consideration is changing strongly as a result of the educational expansion in the Soviet Union. Between 1958 and 1963, basic secondary education became compulsory throughout the Soviet Union and the enhancement of educational attainments across cohorts can still be traced in the data (Table A1). The share of those with at least twelve years of schooling increased from 45 to 50 percent within only two years. However, labor supply and retirement levels are strongly determined by educational attainment. Not accounting for the education composition effects will lead to biases in the estimation of the treatment effects. This problem can be resolved by distinguishing between groups of those holding vs. those without a higher educational degree. Albeit there was a general educational expansion in the USSR, the reforms influencing the cohorts under consideration here are mainly those which increased enrolment of pupils into secondary education.

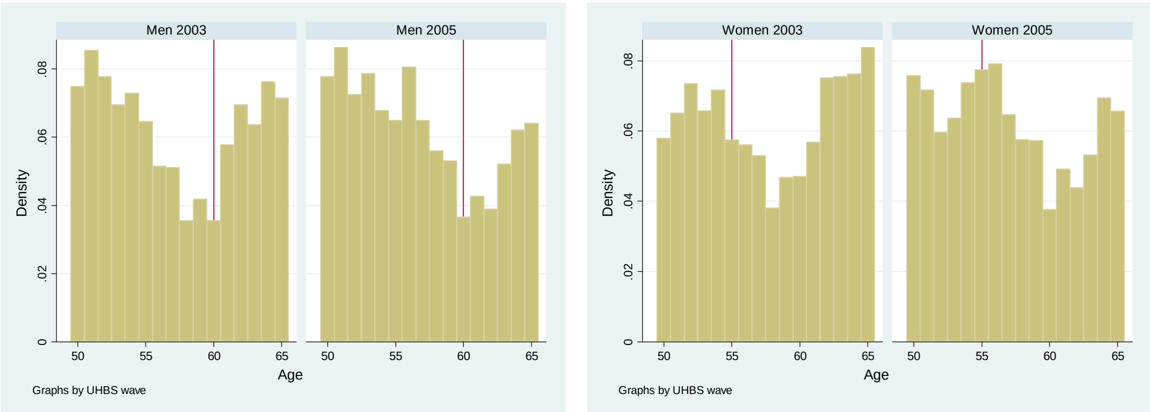
Table A1: Compositional change in educational attainments

Age 45-65	**2002**	**2003**	**2004**	**2005**	**2006**
Average years of schooling	11.3	11.4	11.6	11.7	11.8
Composition shares					
At least 12 years of schooling	44.6	45.4	47.7	49.8	50.1
Higher education	17.2	18.4	18.7	19.3	18.9
Secondary education	61.1	64.0	66.7	68.7	71.0
Lower education	21.8	17.6	14.6	12.0	10.1

Source: UHBS; author's calculation

A second issue of the estimates concerns precision: The density of birth cohorts around the discontinuity threshold is unequal between years, however, this effect is obviously not caused by sorting around the threshold (a main cause for rendering RD applications invalid) but by relatively small birth cohorts during WWII. Therefore, some variation in the densities of observations to the left and the right of the discontinuity threshold prevail. As Figure A1 indicates, the change in densities is especially relevant for men: Between 2003 and 2005, the war-related smaller birth cohorts move from the left side of the discontinuity to the right side, resulting in lower precision of the polynomial regressions below the discontinuity in 2003 and above the discontinuity in 2005.

Figure A1
Observational densities around the retirement age, by gender and survey year



Note: The vertical lines indicate the relevant retirement age for state pensions. The differences in densities do obviously not reflect sorting around the threshold, but reflect different sizes of birth cohorts of the Ukrainian population. For men, the threshold „moves“ through the years of the WWII birth cohorts, producing low densities below (2003) or above (2005) retirement age. Source: UHBS 2003 and 2005; author’s calculations.